MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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H. F. NEWALL, Esq., M.A., F.R.S., PRESIDENT, in the Chair.

Burdett Mason, Larondouette, Bayonne, France; and
Arthur William Meers, F.R.G.S., Lugano, Wickham Road, Beckenham,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended:—

Henry Hermann Gruning, M.Sc., Patent Agent and Engineer, 3 Blakesley Avenue, Ealing, W. (proposed by Rev. F. B. Allison);

Jan B. Hubrecht, Christ's College, Cambridge (proposed by Bryan Cookson);

Victor A. Lowinger, Trigonometrical Survey Department, Taiping, Federated Malay States (proposed by Sir David Gill);

Rev. Malcolm Parker Miller McLean, M.A., The Rectory, West Raynham, Norfolk (proposed by Henry T. Gerrans);

Alfred W. Porter, B.Sc., Assistant Professor of Physics, University College, London, W.C. (proposed by L. N. G. Filon);

Harold Knox Shaw, B.A., Trinity College, Cambridge (proposed by S. A. Saunder);

Capt. Eldred Weston White, Brockley Villa, Upton Road, Southville, Bristol (proposed by E. Fawcett White).

Mr. A. E. Conrady, Mr. R. Inwards, and Mr. G. J. Newbegin were appointed auditors of the Treasurer's accounts for 1906.

Eighty presents were announced as having been received since the last meeting, including, amongst others :—

Abbadia Observatory, Observations, tome 5; and Bordeaux Observatory, Catalogue photographique du ciel, Coordonnées rectilignes, tome 2, presented by the Observatories; Gronnigen Astrographical Laboratory, Libration of the three inner large satellites of Jupiter (W. de Sitter), presented by the Laboratory; Ziegler Polar Expedition, Scientific results, presented by the estate of Wm. Ziegler.

Astrographic Chart; 16 charts presented by the Royal Observatory, Greenwich; and 19 charts presented by the French Minister of Public Instruction.

Solar Parallax Papers. No. 6.

Construction of a Standard Catalogue of Photographic Star Places. By Arthur R. Hinks, M.A.

§ 1. The construction of a standard catalogue of photographic star places along the track of the planet Eros was undertaken in the first instance to provide for the reduction of those series of photographs of the planet which, having a field smaller than 2° square, could not be reduced directly with the *étoiles de repère*; and also to provide exact places of the stars which had been used in visual micrometric comparisons with the planet.

It now seems probable that the standard catalogue may have an extended use; for, as will be shown later, it will be possible by its aid to effect very simply the equivalent of a re-reduction of all the published places of the planet to this new system of stars, lying close to the track of the planet, and more nearly equal to it in magnitude than are the *étoiles de repère*. This should provide a powerful method of searching for systematic errors in the photographic places of the planet and the value of the Solar Parallax derived from them.

§ 2. The published material for the construction of the catalogue was found in the Paris Eros Circulars, Nos. 10 and 11.* The results of an inter-comparison of the work of the different observatories were published in *Solar Parallax Papers* Nos. 4 and 5 (*M.N.*, vol. lxvi. p. 481, vol. lxvii. p. 70, 1906 June and November). In those papers may be found the following conclusions :—

(1) That several series of photographic star places are affected with a magnitude equation of serious amount.

(2) That other series are affected by progressive discordances,

* Circular No. 12 appeared after the principal part of the work was finished. But see later, §§ 11 and 12.

which are sometimes dependent on variations in the adopted places of the fundamental stars, and sometimes not.

(3) That, in general, the star places published in the Paris Circulars are by no means homogeneous.

(4) And that the adopted form of publication did not permit of a complete inquiry into these matters.

§ 3. In 1906 March I ventured to send to the Directors of the observatories concerned a circular letter, submitting for their consideration the following proposals :—

That I should print and distribute a large number of lists of the *étoiles de repère.*

That they should kindly undertake to fill in upon those sheets, in the spaces provided for the purpose, the deduced photographic places of the *repère* stars, using a separate list for each plate, so that the results from each individual plate were kept quite distinct.

That they should also fill in, upon other sheets to be provided for the purpose, the lists for each separate plate of the deduced places of the *étoiles de comparaison* (given in Paris Circulars, Tableau III., either in ledgers or as means, without indication of the plate from which each was derived).

These proposals were very kindly received by the Directors to whom they were addressed. The printed sheets were sent out in 1906 May, and were quickly filled up and returned. My sincere acknowledgments are due to the Directors of the Observatories of Bordeaux, Helsingfors, Northfield, San Fernando, and Toulouse, for the cordiality with which they undertook the troublesome task of filling in upon the printed sheets all the desired information ; to the Astronomer Royal, who sent advance sheets of the Greenwich volume containing equivalent information ; to the Director of the Pulkowa Observatory, who had previously given me similar material in advance of publication ; and to the Director of the Paris Observatory, who furnished advance proofs of the Paris original measures.

The information thus obtained, added to that already published in full in the Paris Circulars for the *étoiles du carré de* 20′, gave me the completely separated results from a very large number of individual plates covering the path of the planet to 1900 December 31. (The discussion of the material for 1901 is postponed to a later date.)

§ 4. To make this considerable mass of material (about 60,000 star positions) homogeneous, I had first to find a way of reducing all the series to one fundamental system,* and to decide what this system should be.

In paper No. 4 I have given reasons for thinking that no one of those systems of fundamental star places based directly upon the meridian observations is satisfactory for our purpose. I decided to form a provisional system from photographic series which had

* See *S.P.P.* No. 4, § 4, for the system to which each was originally reduced.

been reduced to Loewy's system of meridian places. These series are compared in *S.P.P.* No. 5, p. 80, Table VI. It is there shown that the results from Greenwich and Paris are in excellent agreement; that the Toulouse places are nearly accordant with them; but that the Catania places differ from Paris in a large and irregular manner. I decided to omit the Catania results for the time, and to base the provisional photographic system of *repère* stars on a mean of Greenwich, Paris, and Toulouse.

The three observatories were given equal weight. A photographic place depending on 1 or 2 plates was given weight 1; on 3 to 6 plates, weight 2; and on 7 or more, weight 3. There would have been little advantage in adopting a more elaborate system of weights, since the real comparative weight of a result from one and from two plates depends very much upon the consideration whether or not the two plates are upon the same centre, and reduced with the same selection of fundamental stars.

We will call this system the System P.Ph. (provisional photographic).

The rôle of system P.Ph. is temporary. It serves to detect and eliminate systematic deviations between different observatories. It will be judged by the eventual degree of accordance between the reduced results when they are brought together for formation of the mean standard places. If this is satisfactory, we may consider that the system P.Ph. has played its part, and it may be allowed to disappear. We need not give, therefore, in this paper, the adopted places of the *repère* stars in the P.Ph. system, but may proceed at once to examine its use, and the effects of its use.

§ 5. The system P.Ph. is used as follows:—

We take differences between the places of the *repère* stars *derived from a single plate*, and their places in the system P.Ph. The mean of these differences, in the sense P.Ph.—plate, applied as a correction to the plate places of the *repère* stars, makes them conform on the average to the system P.Ph.

The same correction applied to all other star places deduced from this plate should reduce them equally to the system P.Ph., provided that there is no systematic discontinuity between *repère* stars and other stars.

The application of this correction produces in a general kind of way the same effect upon the star places as a re-reduction of the plate to the system P.Ph.; it leaves the results rougher in detail than a complete re-reduction, but nearly without those systematic errors which have been called Progressive discordances in *S.P.P.* No. 5, Tables VI. and VII.

§ 6. Corrections to be applied to individual plates were obtained in this way for the whole of the plates made at Bordeaux, Helsingfors, Northfield, San Fernando, and Toulouse. The following summary will give an idea of the size and distribution of these corrections.

Table I.

Plate Corrections.

R.A. Corr.	Bord.	Hels.	North.	San Fern. Ord.	San Fern. S.S.	Toulouse. Ord.	Toulouse. S.S.
	s	s	s	s	s	s	s
Greatest	+·015	+·027	+·021	+·023	+·034	+·017	+·017
Least	−·010	+·002	−·005	−·017	−·014	−·009	−·021
Mean	+·003	+·014	+·007	+·007	+·009	+·001	+·001
No. greater than +·015	0	16	4	8	6	1	2
between +·015 and +·006	10	15	26	30	18	4	7
+·005 and −·005	21	5	17	19	15	5	22
−·006 and −·015	2	0	0	7	2	6	7
less than −·015	0	0	0	1	0	0	1
Decl. Corr.							
Greatest	+″·18	+″·06	+″·30	+″·30	+″·20	+″·10	+″·19
Least	−·10	−·08	−·12	−·02	−·06	−·06	−·13
Mean	+·04	−·01	·00	+·09	+·07	+·02	+·04
No. greater than +″·15	1	0	1	7	3	0	3
between +·15 and +·06	13	1	8	41	23	5	10
+·05 and −·05	17	33	27	17	14	10	25
−·06 and −·15	2	2	11	0	1	1	1
less than −·15	0	0	0	0	0	0	0

The above figures show that the reduction to a system P.Ph. is by no means superfluous.

The fact that the mean correction for an entire series often differs considerably from zero may no doubt be attributed to divergence in the methods of combining and weighting the meridian results for the adopted *repère* star places.

The variations from plate to plate must be due partly to these divergences, and partly to the roughness of the meridian places compared with the smoothed system P.Ph.

Plate corrections for Catania, Greenwich, and Paris were not derived, because the separated results for each plate were not at the time available.* It seems certain, however, that it would have been superfluous to apply the process to the Greenwich and Paris series, which agree exceedingly well, and have preponderating weight in the formation of the system P.Ph. The absence of plate corrections for Catania is to be regretted, since large divergences occur in this series. (See *S.P.P.* No. 5, p. 84.)

§ 7. *Corrections for Magnitude Equation in the San Fernando and Toulouse Series.*—It was shown in *S.P.P.* No. 5, p. 78, that the Toulouse results have a large and nearly linear magnitude

* The separated results for the Greenwich plates have since been communicated to me in proof, by the kindness of the Astronomer Royal.

equation in declination (but none in right ascension). This result has been confirmed by M. Baillaud, Director of the Toulouse Observatory, but he has not succeeded in discovering the cause of it. (For this information I am indebted to M. Baillaud.)

It was shown in the same place that the San Fernando right ascensions and declinations were both affected by a considerable non-linear magnitude equation. No further light has been thrown on the cause of this error. Corrections were obtained by plotting the results for successive magnitude groups (*loc. cit.*), and drawing the best possible smooth curves through them. The quantities derived from these curves were adjusted to allow for the fact that a general mean correction for stars of all magnitudes resulted from the application of the plate corrections of Table I.: the quantity $0''\cdot03$ was subtracted from the Toulouse corrections, and $0^s\cdot007$, $0''\cdot08$ from the respective San Fernando corrections. The final magnitude corrections applicable to Toulouse and San Fernando are given in condensed form in the following table:—

Table II.

Adopted Corrections for Magnitude Equation.

	Toulouse.	San Fernando.	
	Decl.	R.A.	Decl.
Mag.	$''$	s	$''$
7·0	−0·23	+0·011	−0·21
7·5	− 15	+ 6	− 12
8·0	− 8	+ 2	− 6
8·5	0	− 2	+ 2
9·0	+ 8	− 2	+ 8
9·5	+ 15	+ 2	+ 12
10·0	+ 23	+ 6	+ 15
10·5	+ 31	+ 12	+ 15
11·0	+ 38	+ 18	+ 15
11·5	+ 46	+ 24	+ 14
12·0	+ 54	+ 32	+ 10

§ 8. *Collection of material for formation of catalogue.*—The individual star places published in Paris Circulars 10–12, or communicated to me in manuscript upon the printed forms sent out from Cambridge, were entered on catalogue cards, differently coloured for each observatory. These will be referred to as "detail cards."

The plate corrections and the magnitude equation corrections were applied on the detail cards, and "observatory means" were formed.

Whenever a star had been observed at more than one observatory, a "summary card" was added to the card catalogue on which the observatory means were collected, and the final weighted mean taken.

§ 9. *The System of Weights.*—The summary cards showed at once that the observatories were not entitled to equal weight. At the same time it was not possible to derive a rigorous system of weights from a discussion of the divergences between one observatory and another, or between the individual results of an observatory. These divergences were necessarily much affected by the fact that five or six separate determinations of a star place made at one observatory would depend on the same set of *repère* stars, and be on the same part of the plate, whereas the same star would be determined elsewhere on a different centre.

After some trial it appeared that substantial justice would be done if Greenwich and Paris were given a weight double that of the other observatories, and if the weight of each observatory's contribution, so far as it depended upon the number of observations contributed by each, were on the same scale as was used in making the system P.Ph. of § 4.

Two Catania plates and one San Fernando were rejected for unexplained systematic discordance of large amount; a certain number of obvious misprints in the Circulars were corrected, and a few discordant observations, probably misprints, were excluded.

Finally, stars whose weighted mean had weight 4 or more were passed into a first class, and these form the photographic standard system discussed in what follows. In the region covered by the planet between 1900 Oct. 1 and Dec. 31 the catalogue contains 1300 first class stars. And in addition there are about 2500 stars in a second class, whose weight is less than 4, which are not for the present included in the standard system.

§ 10. *Test of the Standard System.*—As a test of the homogeneity of the contributions which the different observatories make to the standard system, I have discussed the differences, Standard minus Individual contribution to it, from several points of view.

A. The differences, taken with regard to sign, have been grouped (1) in magnitude groups, to see if any residual magnitude equation remained; and (2) in date groups, to see if there were any discontinuity along the path of the planet which might gradually distort the standard system, and damage an ultimate determination of the mass of the Moon.

B. The differences, taken without regard to sign, have been grouped likewise (1) in magnitude groups, to see how far the interagreement of the photographic places is dependent on magnitude; and (2) in date groups, to see if the same standard of internal agreement is maintained along the whole system.

The magnitude groups are those used in *S.P.P.* No. 5, Tables III., IV., and V.

The date groups are broader than those used in Table VII. (*loc. cit.*).

Group	I.	covers	Sept. 15–Oct. 23
,,	II.	,,	Oct. 24–Nov. 24
,,	III.	,,	Nov. 25–Dec. 15
,,	IV.	,,	Dec. 15–Dec. 31

There is no special significance in this particular division, which arose more or less accidentally, and has been retained for convenience.

§ 11. *Search for Magnitude Equation in R.A.*—The division into magnitude groups of the differences Standard minus Individual contribution was made separately for the four date groups. But there is no evidence in the results of any definite change of magnitude equation with the date; if any such change exists, it is obscured by the accidental discordances. Consequently I give only the mean results for the whole period Oct.–Dec., except in the case of San Fernando, to which a correction for magnitude equation has been applied. The separate results for each San Fernando date group are given.

The figures in brackets are the numbers of observatory means contributing to the mean discordances which follow.

Table III.

Comparison of Right Ascensions in Magnitude Groups.

Standard System minus—

Mag.	Bordeaux.	Catania.	Greenwich.	Helsingfors.	Northfield.
	s	s	s	s	s
– 7·4	(14) – ·007	(78) – ·004	(32) ·000	(12) + ·015	(24) + ·012
7·5 – 8·4	(43) – 1	(66) + 3	(73) – 1	(57) + 4	(59) + 3
8·5 – 9·2	(99) 0	(55) + 6	(125) – 1	(75) – 5	(89) – 4
9·3 – 9·8	(48) – 3	(50) + 8	(32) + 2	(89) – 3	(10) – 3
9·9 – 10·5	(131) – 2	(72) + 7	(27) – 5	(117) – 8	...
10·6 – 11·2	(53) – 4	(118) + 3	(5) – 1	(99) – 5	...
11·3 – 12·1	(6) – 15	(167) + 1	(3) – 8	(7) + 6	...
12·2 –	...	(21) – 4	...	...	...

	Paris.	Pulkowa.	Toulouse.	Upsala.	Minneapolis.
	s	s	s	s	s
– 7·4	(47) + ·003	(7) + ·002	(33) – ·003	(15) + ·016	(8) + ·011
7·5 – 8·4	(70) – 1	(22) + 2	(124) + 3	(43) + 6	(17) + 3
8·5 – 9·2	(71) 0	(27) + 1	(229) + 4	(111) + 1	(38) + 2
9·3 – 9·8	(60) – 3	(5) + 10	(99) + 3	(68) – 1	(38) – 13
9·9 – 10·5	(103) + 1	...	(179) + 5	(84) – 1	(49) – 1
10·6 – 11·2	(124) + 2	...	(147) + 8	(33) – 6	(25) – 7
11·3 – 12·1	(193) + 2	...	(49) + 7	(5) – 3	(9) – 15
12·2 –	(267) + 2	...	...	...	(2) – 16

	S. Fern. Group I.	S. F. II.	S. F. III.	S. F. IV.	S. F. Mean.
	s	s	s	s	s
– 7·4	(19) – ·002	(16) – ·012	(4) – ·002	(15) – ·008	(54) – ·007
7·5 – 8·4	(35) + 3	(31) + 4	(9) + 9	(28) + 1	(103) + 3
8·5 – 9·2	(67) + 4	(72) + 2	(42) + 4	(52) + 1	(233) + 2
9·3 – 9·8	(21) + 11	(33) + 2	(21) – 4	(14) + 8	(89) + 4
9·9 – 10·5	(40) + 3	(63) + 11	(29) + 5	(14) – 2	(146) + 6
10·6 – 11·2	(46) 0	(79) – 1	(12) – 3	(8) – 10	(145) – 1
11·3 – 12·1	(8) – 6	(40) + 2	...	(3) – 13	(52) – 1
12·2 –	(7) – 2	...	...	...	(8) 0

The results of this table confirm the conclusion of *S.P.P.* No. 5, that the photographic series which contribute to the standard system are free from relative magnitude equation, with the exception of San Fernando.

The latter observations have been corrected, and we can now see how far this correction has been successful. The correction applied was decidedly non-linear; the above comparison gives some indication that a slight non-linear correction of opposite curvature is now required; that is to say, that the curvature of the original determination was a little excessive. (The material used was not altogether the same.) The magnitude equation results from the separate date groups are not, however, accordant enough to make it probable that any magnitude equation correction for San Fernando is sharply determinable; and we may conclude that the attempt to correct these R.A.'s has been as successful as the material allows.

The Upsala and Minneapolis results were not published in time to be included in the first edition of the standard system. But they show no magnitude equation when compared with it, and may therefore be included safely on a revision.

§ 12. *Search for Magnitude Equation in Declination.*—The declinations have been treated exactly as the right ascensions. Again there is no trace of any definite change of magnitude equation with the date, and the general means are free from it. The corrections applied to the declinations of San Fernando and Toulouse seem to have been successful in eliminating the large errors originally found in them. Table IV. gives the results for the date groups in which the elimination has been least successful, and for the mean.

The lately published declinations of Upsala and Minneapolis show no magnitude equation relative to the standard system.

Table IV.

Comparisons of Declinations in Magnitude Groups.

Mag.	Standard System minus— Bordeaux.	Catania.	Greenwich.	Helsingfors.	Northfield.
– 7·4	(14) +″·06	(78) +″·06	(32) +″·01	(12) +″·02	(23) –″·03
7·5 – 8·4	(43) – 1	(67) 0	(73) – 1	(57) + 5	(59) + 1
8·5 – 9·2	(99) + 1	(57) + 4	(125) + 2	(73) – 1	(89) 0
9·3 – 9·8	(48) + 1	(50) – 3	(32) + 4	(89) 0	(10) – 5
9·9 – 10·5	(131) 0	(74) + 2	(27) + 3	(117) – 2	...
10·6 – 11·2	(53) – 4	(118) + 3	(5) – 3	(99) – 3	...
11·3 – 12·1	(6) – 3	(177) – 6	(3) 0	(7) – 6	...
12·2 –	...	(22) – 11	...	...	...

Table IV.—*continued.*

Comparisons of Declinations in Magnitude Groups.

Standard System minus—

	Paris.	Pulkowa.	S. Fern. III.	S. Fern. IV.	S.F. Mean.
– 7·4	(47) – ″·06	(7) – ″·02	(4) + ″·03	(15) + ″·07	(54) + ″·05
7·5 – 8·4	(70) – 3	(22) + 2	(9) + 6	(28) + 1	(103) + 4
8·5 – 9·2	(71) + 4	(27) 0	(42) + 10	(52) + 5	(233) + 1
9·3 – 9·8	(60) – 3	(5) – 5	(21) + 8	(14) + 9	(89) + 2
9·9 – 10·5	(103) – 2	...	(29) + 5	(14) + 8	(147) + 3
10·6 – 11·2	(123) – 3	...	(12) + 37	(8) + 19	(146) + 8
11·3 – 12·1	(193) – 2	...	...	(3) + 24	(54) + 4
12·2 –	(267) + 1	...	...	...	(8) – 2

	Toul. II.	Toul. III.	Toul. Mean.	Upsala.	Minneapolis.
– 7·4	(9) + ″·13	(9) + ″·07	(33) + ″·05	(15) + ″·06	(8) – ″·05
7·5 – 8·4	(40) – 1	(18) + 3	(124) + 1	(43) + 3	(17) + 9
8·5 – 9·2	(71) – 1	(41) – 9	(229) – 2	(111) + 2	(38) – 1
9·3 – 9·8	(30) + 5	(19) – 7	(97) + 1	(68) + 6	(38) + 3
9·9 – 10·5	(76) + 1	(39) – 1	(179) + 2	(84) + 1	(48) + 9
10·6 – 11·2	(51) – 5	(20) – 17	(147) – 3	(33) + 1	(25) – 5
11·3 – 12·1	(17) – 9	(9) – 21	(49) – 7	(5) + 41	(9) + 9
12·2 –	...	...	...	...	(2) + 10

§ 13. *Conclusion as to Photographic Magnitude Equation.*—As the result of our operations, we have a system of stars derived from photographs made at nine different observatories; and the comparison of each series with the mean shows no relative magnitude equation.

Further, two additional series, not originally included, show no magnitude equation relative to the system.

It is extremely unlikely that these eleven series should be affected by magnitude equation of like sign and amount.

I conclude that our photographic standard system is sensibly free, not only from relative, but from absolute magnitude equation.

§ 14. *Search for Progressive or Uniform Discordances irrespective of Magnitude.*—With the fear of magnitude equation removed, we may examine the mean divergences in the date groups, to see if there is any evidence of progressive or general discordance. The tables which follow are self-explanatory.

Table V.

Comparison of Right Ascensions, in Date Groups.

Standard System minus—

Group	Bordeaux.	Catania.	Greenwich.	Helsingfors.	Northfield.
	s	s	s	s	s
I. Sept. 15–Oct. 23	(117) − ·004	(176) + ·001	(70) − ·002	(76) − ·003	(35) + ·001
II. Oct. 24–Nov. 24	(163) − 3	(290) + 2	(104) 0	(154) − 4	(57) − 1
III. Nov. 25–Dec. 15	(42) − 2	(100) + 9	(52) + 1	(145) − 3	(36) 0
IV. Dec. 16–Dec. 31	(73) − 1	(61) − 1	(71) − 3	(81) − 3	(54) + 1
Mean	(395) − 2	(627) + 3	(297) − 1	(456) − 3	(182) 0

	Paris.	Pulkowa.	San Fern.	Toulouse.	Upsala.	Minneapolis.
	s	s	s	s	s	s
I.	(243) + ·003	...	(243) + ·002	(216) + ·002	(39) − ·002	(42) − ·004
II.	(390) 0	(14) + ·004	(334) + 2	(295) + 9	(149) − 3	(69) − 4
III.	(129) 0	(32) + 4	(118) + 2	(156) + 2	(97) + 18	(33) − 2
IV.	(173) + 2	(15) − 4	(135) − 1	(194) + 2	(74) − 13	(42) − 4
Mean	(935) + 1	(61) + 2	(830) + 2	(861) + 4	(359) + 1	(186) − 4

Table VI.

Comparison of Declinations, in Date Groups.

Standard System minus—

Group	Bordeaux.	Catania.	Greenwich.	Helsingfors.	Northfield.
	"	"	"	"	"
I. Sept. 15–Oct. 23	(117) ·00	(176) − ·05	(70) + ·01	(74) + ·01	(35) + ·02
II. Oct. 24–Nov. 24	(163) − 2	(307) − 2	(104) + 1	(154) − 3	(57) + 1
III. Nov. 25–Dec. 15	(42) + 1	(100) + 1	(52) + 2	(145) − 1	(35) 0
IV. Dec. 16–Dec. 31	(73) + 3	(60) + 16	(71) 0	(81) + 1	(54) − 2
Mean	(395) 0	(643) 0	(297) + 1	(454) − 1	(181) 0

	Paris.	Pulkowa.	San Fern.	Toulouse.	Upsala.	Minneapolis.
	"	"	"	"	"	"
I.	(243) − ·02	...	(243) + ·03	(216) + ·01	(39) + ·09	(42) + ·08
II.	(390) + 1	(14) + ·01	(338) + 1	(294) − 1	(149) − 1	(69) − 2
III.	(129) − 2	(32) 0	(118) + 11	(155) − 6	(97) + 3	(32) + 6
IV.	(172) − 5	(15) − 3	(135) + 6	(194) + 2	(74) + 8	(42) + 4
Mean	(934) − 1	(61) 0	(834) + 4	(859) − 1	(359) + 3	(185) + 3

It is clear that no series has now any sensible uniform discordance from the mean. And the results from the separate date groups are nearly concordant, so that it seems out of the question that the standard system has been seriously distorted in any part by the inclusion of discordant material. The only marked cases of discordance are in Catania group III. and Toulouse group II. We shall have occasion to examine these in a later paper.

§ 15. *Effect of the application of Plate Corrections.*—We may look in the above table for the effect of the application of the plate corrections of § 6. The largest corrections were found for Helsingfors, Northfield, and San Fernando plates.

The Helsingfors plates received on the average corrections of $+^{s}\cdot014$, $-''\cdot01$ (derived from *repère* stars); the results, including all stars, are now discordant from the standard system by $-^{s}\cdot003$, $-''\cdot01$.

The Northfield plates received on the average corrections of $+^{s}\cdot007$, $''\cdot00$; the results are now discordant from the standard system by $^{s}\cdot000$, $''\cdot00$.

The San Fernando plates received on the average corrections of $+^{s}\cdot008$, $+''\cdot08$; they are now discordant from the standard system by $+^{s}\cdot002$, $+''\cdot04$.

In these cases the application of plate corrections has very much reduced the general discordance.

For Bordeaux and Toulouse the mean plate correction was small, and the mean discordance from the standard system remains small.

The discordances of the series to which no plate corrections were applied are also, fortunately, small. We may conclude that the application of the plate corrections has been successful in eliminating such mean discordances between the different series as were sensible.

§ 16. *Influence of Magnitude on the Accuracy of the Photographic Star Places.*

Many of the star images measured upon these plates are at the limit of measurability; and in many cases, by reason of thick skies or unduly short exposures, the images of the planet also are only just measurable. It is therefore very necessary to determine whether the accuracy of the photographic places is seriously impaired when the images are very faint.

To examine this point I have tabulated the average discordances (without regard to sign) of each observatory's series from the standard system. Since the mean discordances are very small, the average discordances will give a fair idea of the relative accuracy of the star places for different magnitudes and for different series.

Table VII.

Average Discordance of Right Ascensions, in Magnitude Groups.

Standard System minus—

Mag.	Bordeaux.	s	Catania.	s	Greenwich.	s	Helsingfors.	s	Northfield.	s
– 7·4	(14)	·014	(78)	·026	(32)	·011	(12)	·019	(24)	·021
7·5 – 8·4	(43)	12	(66)	16	(73)	9	(57)	18	(59)	13
8·5 – 9·2	(99)	11	(55)	22	(125)	7	(75)	17	(89)	14
9·3 – 9·8	(48)	9	(50)	17	(32)	8	(89)	16	(10)	25
9·9 – 10·5	(131)	13	(72)	23	(27)	7	(117)	23	...	
10·6 – 11·2	(53)	9	(118)	21	(5)	6	(99)	23	...	
11·3 – 12·1	(6)	15	(167)	23	(3)	8	(7)	27	...	
12·2 –	...		(21)	22	...		...		...	

Paris.		Pulkowa.	s	San Fernando.	s	Toulouse.	s	Upsala.	s	Minneapolis.	s
(47)	·012	(7)	·010	(54)	·015	(33)	·015	(15)	·030	(8)	·030
(70)	9	(22)	14	(103)	17	(124)	18	(43)	15	(17)	26
(71)	9	(27)	13	(233)	15	(229)	16	(111)	17	(38)	21
(60)	9	(5)	17	(89)	21	(99)	14	(68)	20	(38)	19
(103)	8	...		(146)	24	(179)	18	(84)	21	(49)	21
(124)	12	...		(145)	26	(147)	18	(33)	27	(25)	22
(193)	12	...		(52)	24	(49)	19	(5)	27	(9)	34
(267)	11	...		(8)	19	...		...		(2)	16

Table VIII.

Average Discordance of Declinations, in Magnitude Groups.

Standard System minus—

Mag.	Bordeaux.	″	Catania.	″	Greenwich.	″	Helsingfors.	″	Northfield.	″
– 7·4	(14)	″·17	(78)	″·27	(32)	″·09	(12)	″·19	(23)	″·21
7·5 – 8·4	(43)	10	(67)	22	(73)	8	(57)	19	(59)	10
8·5 – 9·2	(99)	9	(57)	21	(125)	7	(73)	16	(89)	13
9·3 – 9·8	(48)	10	(50)	17	(32)	6	(89)	12	(10)	16
9·9 – 10·5	(131)	11	(74)	20	(27)	8	(117)	16	...	
10·6 – 11·2	(53)	11	(118)	18	(5)	3	(99)	20	...	
11·3 – 12·1	(6)	9	(177)	20	(3)	9	(7)	29	...	
12·2 –	...		(22)	27	...		...		...	

Paris.		Pulkowa.		San Fernando.		Toulouse.		Upsala.		Minneapolis.	
(47)	″·09	(7)	″·07	(54)	″·17	(33)	″·21	(15)	″·18	(8)	″·33
(70)	10	(22)	9	(103)	14	(124)	16	(43)	13	(17)	19
(71)	10	(27)	8	(233)	15	(229)	16	(111)	13	(38)	20
(60)	10	(5)	17	(89)	17	(97)	14	(68)	17	(38)	17
(103)	9	...		(147)	22	(179)	18	(84)	19	(48)	21
(123)	10	...		(146)	27	(147)	19	(33)	21	(25)	22
(193)	12	...		(54)	24	(49)	22	(5)	45	(9)	22
(267)	12	...		(8)	17	...		...		(2)	22

In comparing these results we must remember that Greenwich and Paris have received double weight, and that they will naturally therefore diverge less from the mean than the others. We must also remember that the fainter stars have on the average fewer observations than those of moderate brightness; and no account has been taken of the number of observations for each star at each observatory in making the above comparisons. Nevertheless, the apparent accuracy of the fainter stars falls off very little, except in the case of Helsingfors and San Fernando. The latter series was measured in one orientation only, which may account for much. A more detailed discussion of the Helsingfors series will be given in a later paper, which may help to explain this result.

The general conclusion, that the accuracy of the very faint star places is little inferior to that of the brighter, is as welcome as it is unexpected.

§ 17. *Comparative and Absolute Accuracy of the results from different Observatories.*—Since it has been shown in the last paragraph that magnitude has small influence on accuracy, we may combine the material into date groups without inquiring closely whether the mean magnitude is the same in each group, and may thus discover whether the same accuracy was achieved at different dates, in different parts of the planet's path. And we may further examine whether the rough system of weights adopted fairly represents the relative merits of the results from different observatories.

Table IX.

Average Discordance of Right Ascensions, in Date Groups.

Group	Standard System minus— Bordeaux.		Catania.		Greenwich.		Helsingfors.		Northfield.	
		s		s		s		s		s
I. Sept. 15–Oct. 23	(117)	·010	(176)	·020	(70)	·007	(76)	·025	(35)	·011
II. Oct. 24–Nov. 24	(163)	13	(290)	23	(104)	7	(154)	25	(57)	18
III. Nov. 25–Dec. 15	(42)	12	(100)	22	(52)	8	(145)	14	(36)	19
IV. Dec. 16–Dec. 31	(73)	10	(61)	18	(71)	10	(81)	16	(54)	12
Mean	(395)	11	(627)	22	(297)	8	(456)	20	(182)	15

	Paris.		Pulkowa.		San Fern.		Toulouse.		Upsala.		Minneapolis.	
		s		s		s		s		s		s
I.	(243)	·009	...		(243)	·019	(216)	·015	(39)	·014	(42)	·019
II.	(390)	11	(14)	·011	(334)	22	(295)	21	(149)	20	(69)	21
III.	(129)	13	(32)	15	(118)	21	(156)	16	(97)	25	(33)	32
IV.	(173)	10	(15)	12	(135)	16	(194)	13	(74)	18	(42)	19
Mean	(935)	11	(61)	13	(830)	20	(861)	17	(359)	20	(186)	22

Table X.

Average Discordance of Declinations, in Date Groups.

Group	Standard System minus— Bordeaux.		Catania.		Greenwich.		Helsingfors.		Northfield.	
I. Sept. 15–Oct. 23	(117)	″·10	(176)	″·23	(70)	″·07	(74)	″·18	(35)	″·11
II. Oct. 24–Nov. 24	(163)	12	(307)	·18	(104)	8	(154)	19	(57)	12
III. Nov. 25–Dec. 15	(42)	11	(100)	·21	(52)	6	(145)	13	(35)	14
IV. Dec. 16–Dec. 31	(73)	10	(60)	·27	(71)	8	(81)	18	(54)	14
Mean	(395)	11	(643)	·21	(297)	8	(454)	17	(181)	13

	Paris.		Pulkowa.		San Fern.		Toulouse.		Upsala.		Minneapolis.	
I.	(243)	″·10	...	″	(243)	″·17	(216)	″·16	(39)	″·19	(42)	″·20
II.	(390)	10	(14)	·06	(338)	20	(294)	18	(149)	16	(69)	18
III.	(129)	15	(32)	11	(118)	21	(155)	19	(97)	18	(32)	20
IV.	(172)	11	(15)	9	(135)	18	(194)	16	(74)	15	(42)	25
Mean	(934)	11	(61)	9	(834)	19	(859)	17	(359)	17	(185)	21

We may draw the following conclusions from these tables :—

The concordance of the observations is fairly uniform along the path of the planet, though there are considerable exceptions in the R.A.'s from Helsingfors, Minneapolis, Toulouse, and Upsala.

The Greenwich and Paris places were fully entitled to the double weight which they received.

The Bordeaux places might also have received double weight.

The Northfield and Pulkowa places might have been given some extra weight. But as they are chiefly well-observed *repère* stars, the effect upon the standard system would have been small in any case.

Judged by their average discordance, the Catania and San Fernando places are barely entitled to the unit weight which they received. Attention has already been called to very large and irregular discordances in the former series (*S.P.P.* No. 5, p. 84). The San Fernando plates have generally only a single exposure, measured in a single orientation, whereas other observatories usually have three or four exposures, measured in at least two orientations.

A second of time is equivalent to ten seconds of arc in declination 48°, which is close to the mean declination of the stars in our standard system. A comparison of the figures in the above table shows that the right ascensions and declinations are very closely of the same average accuracy.

§ 18. *Probable Error of the Standard System.*—To obtain from the above discussed discordances an exact estimate of the probable error of a star place in our standard system would be very laborious, if not impracticable, and the labour is not warranted in the present stage of the work.

We may, however, estimate the probable error roughly, as follows :—

The average discordance of the contributions to which unit weight has been given is well under 0″·20. A majority of these depend upon one or two observations, and have received weight 1. The probable error of a contribution of weight 1 is likely to be not greater than 0″·20, probably less. And a star is not admitted to the standard system unless its weight is at least 4, while the average weight is 7 or 8. The probable error of a standard place in each co-ordinate will therefore be well under 0″·10, but how much under it is impossible to say at present.

§ 19. *The use of the Standard System.*—The standard system provides fairly completely for the reduction of the Crossley Reflector plates, furnishing from 8 to 12 comparison stars along a good part of the planet's path. These star places have been already communicated to Professor Perrine. There are thin places in the catalogue, due to bad weather in Europe while it was fine at Mount Hamilton ; these must be filled up if possible. By the kindness of the Astronomer Royal, a considerable part of the material required has been already supplied from special measures made on Greenwich plates, which are not included in the above discussion.

The catalogue also provides for the complete reduction of the Cambridge, Oxford, and Tashkend series, and any others that may be outstanding.

Further, I propose to try the effect of making the equivalent of a re-reduction of the published photographic places, in the following way :—

The programme of Monsieur Loewy included the measurement of all stars in a square of 20′, having the planet at its centre ; these were reduced and published separately for each plate. The "*carré de vingt minutes*" generally contains six or eight of our standard system stars, sometimes as many as twenty, and we can compare the individual plate places with those in the standard system. If anything abnormal has happened to the plate, either in making or in reduction, the effect upon the mean of a group of stars scattered over the square would be very nearly the same as upon the planet at its centre. And if we apply to the planet's place a correction derived from the mean difference Standard System minus Stars of the Square, it seems that we ought to get rid of any troubles of a systematic nature in a very simple way.

Suppose, for example, that one telescope has an optical distortion that displaces the centre of the plate with respect to its edges ; that another has an optical distortion that skews the plate and makes the scale value in x and y sensibly different, so that there is a large and systematic difference between a six constant and a four constant linear reduction ; suppose that a third observatory has used a quite wrong expression in calculating the second order refraction corrections ; and that in the work of a fourth the errors in the solution of the plate are undesirably common,—and these suppositions are not so extravagant as they may appear,—the simple

process of reduction to the standard system by means of the stars in the 20′ square will avoid the systematic effects of all these untoward circumstances, and will give a result equivalent to, though not so smooth as, a complete re-reduction.

The standard system will also be indispensable for the reduction of the micrometric comparisons of the planet. In Paris Circulars 8 and 9 Monsieur Loewy published a list of stars used in these comparisons, and desired that they should be measured on the photographs whenever possible. As a result of this, the standard catalogue contains good places of a very large majority of these stars, and provides almost completely for the reduction of most of the micrometer series. The only serious difficulty may be found in reducing the observations made at Washington and Yerkes Observatories, where they used some stars so faint that they are not found on the ordinary photographs. These stars were all marked on charts sent to Professor Perrine for use in selecting his comparison stars, and he has succeeded in finding and measuring a large part of them on the Crossley plates.

We have therefore reason to hope that this standard photographic system of stars may provide, directly or indirectly, for the complete reduction of the Eros observations of 1900 on a homogeneous system.

The extension of the system to the early months of 1901 is making excellent progress.

§ 20. As before, the additional assistance required in this work has been provided by a grant from the Government Grant Fund of the Royal Society.

Acknowledgments are also due to Miss Julia Bell, Girton College, who has continued to perform a great part of the computations; and to S. E. Bowd, who has made the card catalogue and assisted efficiently in all parts of the work.

Cambridge Observatory:
1907 *November* 19.

Note on the Position of the Sun's Axis of Rotation, as deduced from Greenwich Sun-spot Measures 1886–1901. *Papers of the I.U.S.R. Computing Bureau, No. I.* By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

1. The present note is to be regarded rather as a statement of problems than as a solution of them. A discussion of the Greenwich sun-spot measures was begun a few years ago, as a necessary preliminary to computations in connection with the International Union for Solar Research; and it is being continued as the work of the Computing Bureau, of which the writer was asked to take charge. But the resources of this Bureau are at present very slender, and the time of the Director is much occupied with the printing of the Astrographic Catalogue, so that it may be some time before a definitive discussion is completed. Meanwhile the following provisional statement may be of interest; it has been drawn up in response to one or two inquiries for information. Another note will shortly be presented, dealing with the velocity of rotation.

2. The material used is taken from the Greenwich sun-spot ledgers, 1886–1901 inclusive. Later publications, especially the welcome volume of "photoheliographic results, 1874–1885," which has just been issued, will allow of a considerable extension of the discussion; but these were not available when the following computations were commenced. The discussion was confined to spot groups which were seen for a period of ten days at least; and no notice is taken in what follows of the identity of recurring groups. The longitude and latitude on each day were compared with the mean values given in the ledgers; and the differences were tabulated under each 10° of distance from central meridian, so as to show the motions in latitude and longitude. The spots were further grouped according to their latitudes, in groups 0° to 10°, 10° to 15°, 15° to 20°, 20° to 25°, and over 25°, N. and S. latitudes being kept separate.

3. When we have a set of mean residuals for each 10° of longitude, various methods may be proposed for deducing the mean drift from the series. Take, for instance, the following series of mean residuals in latitude for spots in N. latitudes 0° to 10° in the month of October (all years combined).

Dist. from Central Merid.	Mean Resid.		Dist. from Central Merid.	Mean Resid.	
	°			°	
−90° to −80°	+0·50		+90° to +80°	—	
−80 to −70	+ ·28		+80 to +70	−0·38	
−70 to −60	+ ·73	+0·40	+70 to +60	− ·32	−0·38
−60 to −50	+ ·20		+60 to +50	− ·44	
−50 to −40	+ ·27		+50 to +40	− ·33	
−40 to −30	+ ·23	+ ·22	+40 to +30	− ·04	− ·24
−30 to −20	+ ·16		+30 to +20	− ·34	
−20 to −10	+ ·15		+20 to +10	− ·36	
−10 to 0	− ·04	+ ·06	+10 to 0	− ·12	− ·24

Differences of method would turn chiefly upon the relative weights to be assigned to the outer and inner residuals. The outer give better intervals for measuring the motion; but, on the other hand, they are affected by larger errors, owing to foreshortening, and possibly to optical distortion. We shall do no harm by making two separate determinations of drift from outer and inner groups and comparing the results. If we reject the outermost groups (80° to 90°) as too much foreshortened, and take the mean of the next three, we get results for +65° and −65°, a difference of 130° in longitude. The next three groups will give us mean values for +35° and −35°, a difference of 70°.

4. A drift in longitude may be taken to increase steadily with the time, and hence the differences for ±65° should be to those for ±35° in the ratio of 13 to 7. Thus, if the above figures had related to longitudes, the mean drift per 10° would have been $(-0°·38-0°·40)/13=-0°·060$ from the outer groups and $(-0°·24-0°·22)/7=-0°·066$ from the inner groups.

5. The same might be true of a drift in latitude if it were a physical drift; but if it is an apparent drift, due to a faulty determination of the Sun's axis, these formulæ will not apply. The error due to a wrongly assumed axis is of the form $k \sin(\theta+65°) - k \sin(\theta-65°) = 2k \cos\theta \sin 65°$ in one case, and (similarly) $2k \cos\theta \sin 35°$ in the other. The ratio is thus $\sin 65°/\sin 35° = 1·58$ instead of $65/35 = 1·86$.

6. To take the latitude first: if we have adopted a wrong axis, the effect will be a spurious latitude drift, varying in amount as we go round the Sun during the year. The drift will in fact be, roughly, similar to the Sun's drift in declination, if for a moment we regard the equator as an erroneous determination of the ecliptic: it will be northwards at one time of year and southwards six months later, with no drift midway. Hence, if we arrange the results according to the time of year, they will form a cycle. The results for latitude were accordingly collected in months, each spot being assigned to the calendar month in which it appeared on the central meridian; and the mean drifts between the meridians above specified are given in Tables I. and II., which require no further explanation.

TABLE I.

Latitude drift between $+65°$ *and* $-65°$ *in each month* (*of the years* 1886–1901).

Latitude.	$-25°$	$-20°$	$-15°$	$-10°$	$0°$	$+10°$	$+15°$	$+20°$	$+25°$
Jan.	+78	−94	−77	−25	−23	−85	−87	−33	−32	−88
Feb.	+71	−67	−13	−7	−29	−7	+1	+30	−118	—
Mar.	−67	−15	−20	−39	−37	−43	−33	−76	+25	+186
Apr.	−28	—	+17	+14	−4	−19	−3	+22	+13	—
May	+15	+76	−54	+31	+77	−5	0	+2	−29	—
June	—	+5	−54	−17	+74	−20	−17	−2	+33	+120
July	+115	−18	+42	−75	+18	−19	−25	+93	−145	+270
Aug.	−43	−27	−12	−37	+6	+90	−41	−59	+16	−25
Sept.	+115	−29	−53	−13	−28	−33	−43	+13	+50	−12
Oct.	−113	−115	−7	−16	−3	−78	−75	−115	−188	−40
Nov.	−15	−159	−52	+33	−41	−59	−124	−116	−148	
Dec.	−82	−66	−57	−112	−13	−57	−119	−29	+70	+180
Mean	—	—	−28	−22	0	−28	−47	−22	—	—

The unit in the table is $0°{\cdot}01$, and the figures are differences between the mean residual at $+65°$ and that at $-65°$. The row of figures at the top represents solar latitude.

TABLE II.

Latitude drift between $+35°$ *and* $-35°$ *in each month* (*of the years* 1886–1901).

Latitude.	$-25°$	$-20°$	$-15°$	$-10°$	$0°$	$+10°$	$+15°$	$+20°$	$+25°$
Jan.	+30	+5	+20	−18	−42	−39	−48	−25	+87	−5
Feb.	+32	−30	−8	−16	−6	−26	+12	+22	−65	+25
Mar.	−35	−3	−9	−26	−19	−23	−20	−28	+30	+54
Apr.	−22	+107	+3	+9	+20	−15	−4	−15	+3	—
May	+28	−21	−50	+5	+22	+34	+25	+45	−42	—
June	−85	−27	−25	−30	+58	+4	−6	+6	+49	+20
July	+25	+15	+38	−6	−5	−29	−18	+50	−101	+165
Aug.	+22	−25	−12	−57	−6	+30	−39	−25	+15	+35
Sept.	+118	−5	−40	−9	+28	−1	−10	−37	+33	+32
Oct.	+50	−30	−55	−5	+7	−46	−38	−19	−193	+10
Nov.	+55	−54	−42	+53	−33	−16	−61	−86	−85	...
Dec.	−98	+10	−25	−57	−48	−67	−32	−29	−43	+125
Mean	—	—	−17	−13	−2	−16	−20	−12	—	—

Multiplying the coefficients A and B by $\frac{1}{2}$ cosec $65°$ for Table I., and $\frac{1}{2}$ cosec $35°$ for Table II., we get the error of the Sun's axis,

which we may now express (as Carrington does) in minutes of arc. The separate determinations are given in Table III.

Excluding, at any rate in the first instance, the outside groups (for latitudes above 20°), we can now analyse the remaining 6 columns in each table harmonically, to find an expression of the form—

$$A \sin \theta + B \cos \theta + C,$$

where $\theta = 0$ for the middle of January.

TABLE III.

Quantities representing the Errors of the Sun's adopted axis.

Latitudes.	−20°	−15°	−10°	0°	+10°	+15°	+20°	Mean.
A from Table I.	+2·9	+3·7	+6·6	+4·2	+14·5	+12·1		+7·3
A ,, ,, II.	+7·1	−0·8	+6·5	+5·7	+12·1	+14·5		+7·5
B from Table I.	−5·8	−1·0	−11·6	−12·0	−9·5	−9·7		−8·3
B ,, ,, II.	+0·3	+1·3	−16·7	−13·7	−4·9	−10·6		−7·2

Inspection of Table III. suggests the following conclusions:—

(*a*) The determinations from meridians ±65° and ±35° are satisfactorily accordant. Thus the Sun's disc may be treated as free from distortion for the purpose in view.

(*b*) The adopted axis is sensibly in error for the period under discussion.

(*c*) The different zones give sensibly different values for the error, zones −20° to −15° and −15° to −10° being specially discordant. But the results seem really to fall into three groups:

	−20° to −10°	−10° to +10°	+10° to +20°
A	+3·2	+5·8	+13·3
B	−1·3	−13·5	−8·7

The conclusion (*c*) was not altogether unexpected, for an inspection of Carrington's material had indicated something of the kind. The material is given on pp. 240–242 of his volume *Observations of Solar Spots* (published in 1863 by Williams & Norgate); and if it is divided into four groups according to the latitude of the spots, we get approximately for his X and Y—

Latitudes.	X	Y
> +15°	− 3·8	+12·9
+15° to 0°	− 9·4	+10·2
0° to −15°	− 4·1	−15·4
> −15°	−15·0	+ 9·0
Mean	− 8·1	+ 4·2

It thus seems doubtful what precise meaning can be attached to the mean of such discordant values. On forming groups corresponding to those used in this paper there are still clear discordances; but the whole question must be reserved for further examination.

Nothing has yet been said of the constant C indicated by the mean values of the columns. It will be seen that it is persistently negative, the mean value from Table I. being $-0°{\cdot}25$, and from Table II. $-0°{\cdot}13$. These values are consistent, and indicate a steady drift of $-0°{\cdot}019$ or $-1'{\cdot}14$ for every 10° of advance across the disc. Carrington found a phenomenon of this kind, and thought its reality so improbable that he set it down to some error in reduction. His value was $1'{\cdot}5$ for 30° of rotation, or $18'$ for a whole rotation; and his direction was *towards* the North Pole, *i.e.* in the direction opposite to that here found. It thus becomes important to examine whether this general drift shows any clear indication of change during our period (1886–1901). For this purpose the material must be rearranged in consecutive years, as in Table IV.

TABLE IV.

Drift in Latitude between +65° and −65° from central meridian.

Year.	−25°	−20°	−15°	−10°	0°	+10°	+15°	+20°	+25°
1886	—	—	-4_{9}	-13_{7}	$+8_{14}$	-14_{3}	$+27_{2}$	-57_{2}	—	—
1887	—	—	—	-70_{1}	$+29_{10}$	-4_{4}	-65_{3}	—	—	—
1888	—	—	—	-128_{2}	-102_{6}	$+110_{1}$	-145_{1}	—	—	—
1889	-2_{2}	$+30_{1}$	-60_{1}	—	-66_{5}	$+25_{1}$	—	—	—	—
1890	—	-68_{1}	—	—	-21_{2}	—	—	$+65_{1}$	-33_{1}	$+60_{1}$
1891	$+51_{2}$	$+98_{4}$	-27_{9}	$+35_{1}$	—	—	$+19_{6}$	-12_{17}	-41_{13}	$+42_{5}$
1892	$+2_{10}$	-114_{15}	-30_{13}	-26_{9}	$+57_{3}$	-35_{9}	-55_{26}	$+6_{10}$	$+5_{7}$	$+119_{5}$
1893	$+61_{4}$	-43_{11}	-47_{20}	-24_{26}	$+21_{23}$	-16_{10}	-19_{15}	-4_{17}	-83_{8}	-25_{1}
1894	$+47_{5}$	-25_{1}	-40_{16}	-50_{20}	$+3_{17}$	-11_{22}	-65_{18}	-7_{15}	$+4_{4}$	—
1895	—	$+63_{4}$	-49_{10}	-50_{18}	-31_{14}	-68_{5}	-48_{25}	-37_{14}	-26_{4}	—
1896	—	-42_{5}	-4_{10}	-16_{16}	$+16_{4}$	-95_{5}	-127_{3}	-122_{5}	-15_{1}	—
1897	—	—	-5_{2}	$+33_{6}$	-24_{19}	-77_{14}	-79_{5}	—	—	—
1898	—	—	—	$+3_{9}$	-26_{11}	-109_{6}	-100_{6}	—	—	—
1899	—	—	—	$+20_{7}$	$+40_{5}$	-23_{2}	—	—	—	—
1900	—	—	—	$+60_{1}$	-22_{6}	$+5_{2}$	—	—	—	—
1901	—	$+117_{1}$	—	—	—	$+17_{2}$		—	—	—

The unit in the table is 0°·01 : the subscript figures represent the number of spots included in the mean.

The mean values of the drift, weighting all spots equally, are as follows :—

TABLE V.

Year.	Mean Drift.	No. Spots.	Year.	Mean Drift.	No. Spots.
1886	−0°·04	37	1894	−°·24	118
1887	·00	18	1895	− ·40	94
1888	− ·90	10	1896	− ·39	49
1889	− ·34	10	1897	− ·39	44
1890	− ·03	6	1898	− ·47	32
1891	− ·02	57	1899	+ ·21	14
1892	− ·30	107	1900	− ·07	9
1893	− ·19	135	1901	+ ·50	3

There are some indications of change, but the small number of observations in the terminal years makes it difficult to pronounce with certainty. The reduction of the material recently made available should go far to settle the point. Meanwhile we may sum up the indications by taking means for four years as follows, Carrington's result being rendered comparable and added :—

Mean drift during a whole rotation.

1853–1861 Carrington.		1886–1889	1890–1893	1894–1897	1898–1901
+ 18′	***	− 30′	− 32′	− 56′	− 30′

[NOTE, added December 28.—A provisional reduction of the results for 1874–1885 shows that the drift is distinctly *positive* in those years, being smaller at the ends and large in the middle. An oscillation of about 26 years' period with maximum northerly drift about 1854, 1880, 1906, and maximum southerly about 1867, 1893, would roughly fit the facts observed hitherto.]

On the Mean Distances of the Groombridge Stars.

By A. S. Eddington, B.A. M.Sc.

In a former paper * I have given some calculations as to the mean distance of the stars of Groombridge's catalogue for the purpose of comparing the distances of the two star-drifts, with which that paper dealt. The results obtained were, I believe, sufficient to show that the two drifts must be at, roughly, the same mean distance from us, but there seemed no means of judging how closely the actual numerical results could be relied on. Some further reflection has, however, shown that the method has advantages (as regards freedom from systematic error) which were not at that time noticed; it also incidentally furnishes an additional test of the two-drift hypothesis. I have accordingly developed the results more precisely in this paper.

The principle of the method of determining the mean parallax of a large group of stars is well known. It depends on determining the apparent angular displacement corresponding to a known linear displacement. For example, in Professor Kapteyn's researches the parallactic motion has been mainly used as the known linear displacement; if that is assumed to be the same for all the different groups of stars discussed, the corresponding angular motion is proportional to the mean parallax of the group. In the present case the mean peculiar speed (*i.e.* the mean individual motion, irrespective of direction, which remains after all drift or parallactic motion has been abstracted) is taken for a similar purpose. If its linear amount is assumed to be the same in all parts of the heavens, the angular amount will be proportional to the mean parallax of the stars. We must, of course, be prepared for the possibility that neither the parallactic motion nor the mean peculiar speed is strictly constant in different parts of the sky or for different classes of stars. The determinations of "hV" (or the ratio of the drift motion to the mean peculiar speed) made in the previous paper throw some light on this question; they appear to favour the assumption of constant mean peculiar speed, but the test is at present a rather rough one.

As the proper motions of the stars include not only their peculiar motions but also the drift-motions, the mean peculiar motion cannot be found without mathematical investigation. The theory developed in the previous paper enabled this to be done. There are two stages in the calculation:—(1) The constants of the drifts must be determined from the numbers of stars moving in the different directions (without regard to the magnitudes of their proper motions); and (2) from these constants the theoretical relation between the mean peculiar motion and the whole mean motion can be found.

For example, consider that division of the sky which I have

* *Monthly Notices*, vol. lxvii. p. 34, referred to hereafter as "*Systematic Motions.*" The portion especially dealing with mean distances is pp. 55–57.

called Region B. In fig. 1, A is for this region the theoretical curve of the kind considered in the former paper, in which the radius vector is proportional to the *number* of stars moving in the corresponding direction; B is the theoretical curve in which the radius vector is proportional to the *mean proper motion* of the stars moving in the corresponding direction. The two curves are rather similar; the bi-lobed character of B is readily recognised, although the elongations and minima are much less pronounced than in A.

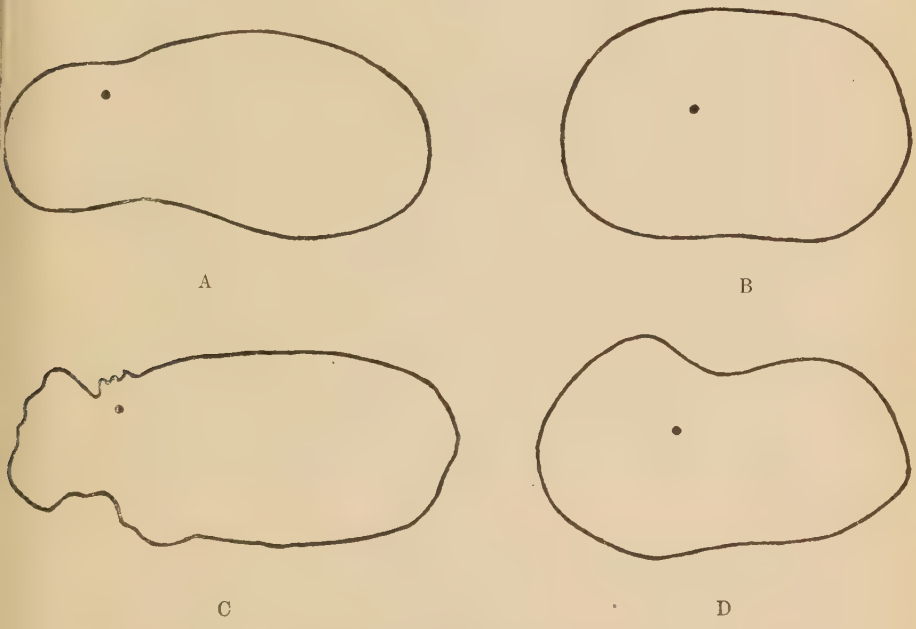


FIG. 1.—Diagrams for Region B.

A and C—theoretical and observed curves, showing total number of proper motions in different directions.

B and D—theoretical and observed curves, showing mean proper motion in different directions.

It should be mentioned that in drawing B the two drifts have been assumed to be at the same mean distance; as the two lobes of the curves correspond respectively to stars of the two drifts, it is clear that if one of the drifts is nearer than the other, this will be shown by the lobe of the curve B, which corresponds to that drift, being exaggerated in size compared with the other. It is in this way that the mean distances of the drifts can be compared. The curves C and D are those derived from observation, and are to be compared respectively with the theoretical curves A and B. Although our main purpose in this investigation is to discuss the distance of the stars of the two drifts, attention may be called to

the clear way in which, in this and in other regions, the mean proper motions support the two-drift hypothesis. The evidence of the observed curves C and D respectively in favour of that hypothesis must be regarded as, at least to a large extent, independent; it could hardly be expected, *a priori*, that the mean proper motions would be greatest in the two directions in which the total number of proper motions is greatest. It may be noticed that the two drifts are slightly more prominent in the observed curve D than in the theoretical curve B; some of the other regions considered show this rather more strongly, and it appears to be a general result. I have not been able to arrive at any satisfactory explanation of it.

For the other regions, reference must be made to Table I. In it will be found, for each of the seven regions covered by the Groombridge catalogue, the mean proper motion of stars moving in each of twelve directions at intervals of 30°. (For forming the mean proper motion in any direction, stars moving in directions within 15° on either side of it were used; thus each direction corresponds to a 30° sector.) Curves similar to D in fig. 1 could readily be constructed from these data, and we should find that they are clearly bilobed, indicating two drifts. Region D constitutes an exception; for, since it contains the apex of Drift II., that drift is not very apparent in it. The same thing may be seen by inspecting the numbers in the table: in each column there are two maxima and two minima well indicated, and a comparison with the Tables II. to VII. in "*Systematic Motions*" shows that they correspond with the maxima and minima in the numbers of stars moving in the different directions.

TABLE I.

Mean Proper Motions (rejecting highest eighth).

Direction θ. Regions A and B	Regions C D E F and G	Region. A	B	C	D	E	F	G
°	°	″	″	″	″	″	″	″
5	15	5·65	4·64	2·75	3·23	4·22	2·99	2·29
35	45	3·99	3·37	4·22	3·54	3·85	4·72	2·11
65	75	3·00	2·40	3·80	4·72	5·24	6·76	2·64
95	105	2·74	2·51	2·61	5·04	4·72	3·49	2·93
125	135	3·11	2·57	3·34	5·11	5·86	3·20	2·87
155	165	3·35	2·90	2·82	3·76	6·42	2·94	2·09
185	195	3·32	2·54	2·74	3·15	6·90	4·47	1·70
215	225	4·28	2·21	1·53	2·10	4·95	5·94	1·94
245	255	3·50	2·14	1·94	2·71	4·33	4·88	2·92
275	285	2·49	1·54	1·61	2·07	2·06	4·39	3·53
305	315	3·80	1·46	1·32	2·00	2·77	3·81	4·40
335	345	4·84	3·60	2·50	3·76	5·49	2·98	2·81

In calculating these means, it is necessary to adopt some rule for rejecting excessive proper motions. The difficulty is to find a limit which will affect the two drifts equally. The rule which

I have adopted, which seems to meet this, has been to reject the fastest eighth moving in each direction. That is to say, if there are n proper motions in one of the twelve 30° sectors, I pick out and reject the $n/8$ highest of these, and take the mean of the remainder. The net result must be nearly the same as if the nearest stars of each drift, to the number of rather more than one-eighth of the whole, were rejected. This means a much more drastic rejection of high proper motions than is usually resorted to; but some such plan seems necessary if the more distant stars are not to be altogether outweighted.

Each of the mean proper motions provides us with an equation involving linearly the unknown mean parallaxes of the two drifts (see "*Systematic Motions*," p. 56, equation (1)). Thus for each region twelve equations were formed, involving these two unknowns; these were properly weighted and solved by least squares. Table II. contains the results. "d_1" and "d_2" denote the distances of the drifts. For the meaning of "h" reference must be made to the mathematical analysis of the former paper; it is connected with the mean peculiar speed of the stars. It is, however, the relative rather than the absolute values of the distance that are of interest, so that the precise unit used is not of great importance; but, accepting Campbell's provisional value (20 k.m. per sec.) for the speed of the solar motion, and my own determinations of the drift-constants, the actual mean parallaxes may be obtained by dividing the numbers in Table II. by 500. (It must, however, be remembered that a great many near stars have been rejected, as explained above, so that the mean parallax has a rather artificial significance.)

TABLE II.

Mean Parallaxes.

(The quantities $\frac{1}{hd_1}$ and $\frac{1}{hd_2}$ tabulated are the mean parallaxes multiplied by a factor whose value is probably about 500.)

Region.	*N.P.D.*	R.A.	$\frac{1}{hd_1}$.	$\frac{1}{hd_2}$.
	° °	h h	" "	" "
A	0–20	0–24	2·96±·07	3·41±·13
B	20–52	22– 2	2·45±·07	2·40±·11
C	,,	2– 6	2·39±·08	2·65±·12
D	,,	6–10	3·35±·12	3·23±·21
E	,,	10–14	3·65±·14	4·78±·29
F	,,	14–18	3·74±·23	4·15±·31
G*	,,	18–22	2·77±·16	2·55±·18

The equality of distance of the two drifts is evidently confirmed. Region E is the only one in which there is any indication of a considerable difference between their distances. The probable errors are of interest, not only as indicating the reliability of the

* The restricted region G (excluding subdivision γ) was used; see *Systematic Motions*, p. 52.

results found, but as a measure of the accordance between theory and observation, for they are derived from the discordance of the calculated and observed mean proper motions in the twelve directions. Region B, which was represented in fig. 1, shows the best agreement between theory and observation; but it may be remarked that, where the agreement is not so good, that is always due to the phenomenon mentioned above—that the mean proper motions show the effect of the two drift-motions more prominently than calculation leads us to expect.

Before we can rely on the values of the probable error given in Table II., it is necessary to consider whether a possible systematic error in the catalogue, from which the proper motions are derived, may not lead to errors of importance not included in the theoretical probable error. The conclusion is rather unexpected; a systematic error in the proper motions does not invalidate these results. The reason is this: a systematic error affects the mean proper motion, and also the total number of proper motions, in a particular direction; owing to the latter error, we arrive at incorrect values of the constants of the drifts; but it is not difficult to see that, if these incorrect values of the drift-constants are used in forming the equations of condition, this will compensate for using incorrect values of the mean proper motions. Looked at in another way, the analysis was designed to extricate the mean peculiar motion from the systematic drift-motions with which it is involved; it at the same time extricates it from any possible systematic error which is hardly distinguishable in its effect from systematic drift or parallactic motion. In order that this elimination of error may take place, it is necessary to use drift-constants derived from the observed proper motions in the particular region considered, not those determined from an examination of the whole sky; this has been attended to.

Assuming that the two drifts are actually at the same mean distance, and accordingly combining their parallaxes given in Table II. with weights inversely proportional to the squares of their probable errors, we have the following table of mean parallaxes (multiplied by a factor) of the Groombridge stars in different parts of the sky.

TABLE III.

Variation of the Mean Parallax with Galactic Latitude.

Region.	Mean Galactic Latitude.	Mean Parallax (multiplied by a factor).
	°	″
B	−12	2·43
C	− 2	2·47
G	+ 8	2·67
A	+27	3·06
D	+32	3·32
F	+46	3·88
E	+66	3·87

The mean parallax steadily increases with the distance from the galaxy, a result which is in accordance with the generally accepted ideas of the distribution of stars, viz. that the increased number of stars in the low galactic latitudes is due to additional more distant stars being visible, and not to any crowding among the nearer stars. The table apparently indicates a *gradual* change in the mean distance; but, as the regions are rather extensive in area, it is not definitely incompatible with the hypothesis that the differences are caused by a belt of distant stars almost limited to the galaxy.

On Ancient Eclipses. By P. H. Cowell, M.A., F.R.S.

I am much obliged to Mr. Crommelin for repeating my calculations in the case of six solar eclipses, and so setting the question of their accuracy beyond doubt.

The fact that emerges from the discussion of the eclipses is that there is an unexpected rate of change in what may be termed the "nodical year," or period of revolution of the Sun relatively to the Moon's node.

This abnormal rate of change may arise from secular changes in the motion of the Moon's node (other than that arising from the change of the eccentricity of the Earth's orbit round the Sun), or from secular changes in the motion of the Sun, or, of course, from a combination of both hypotheses.

The motion of the node, as found from observation, differs very slightly from the formula given for it by Professor Brown in *M.N.*, lxiv. p. 532. This formula is differentiated and the secular acceleration of the node obtained on the supposition that the eccentricity and inclination of the Moon's orbit are constant. We know no reason why these quantities should vary, and observation shows that if they do vary, the variations do not exceed certain limits. It happens, however, that these limits are sufficiently large to admit of a considerable part of the unexpected change in the nodical year being attributed to the motion of the node (at least as an alternative hypothesis), thus diminishing the part to be ascribed to the Sun.

The formula $+4''(T-18{\cdot}25)^2-1''{\cdot}0$ takes numerical values $+1''{\cdot}0$ in 1755 and 1895, and $-1''{\cdot}0$ in 1825, with lesser values numerically at intermediate dates. A second of arc is not an impossibly large quantity to attribute to systematic errors, or even to unknown long-period terms, and it is clear from Professor Newcomb's table (*Ast. Const.*, p. 22) that residuals of over 1″ cannot be avoided by any formulæ.

Hence it must be understood that a possible change in the eccentricity and inclination of the Moon's orbit is put forward as

an alternative suggestion. There are no modern observations capable of discriminating between various hypotheses, and, conversely, it is unnecessary to discriminate before approaching questions of chronology. All that is necessary in that case is to verify that certain empirical formulæ fit the records in a way that cannot reasonably be considered accidental.

It is a curious reflection that Oppolzer's formulæ should have been put aside merely because they contain impossible mean motions. Because they are based on so much coincidence and because Oppolzer's numerical accuracy is beyond reproach, there must be some solid foundation beneath the superficial blemishes of impossible mean motions. If we discard Oppolzer's position of the lunar perigee as not sensibly affecting the general agreement with the eclipse records, and if, wherever a mean motion needs correction, we apply to Oppolzer's formulæ some multiple of $20T + T^2$, a quantity that vanishes twenty centuries ago, we get to results closely resembling mine; and I attribute the small differences to the fact that Oppolzer nowhere claims to have satisfied but merely to have improved the solar eclipses, and, I believe, he obtained his formulæ from the lunar eclipses.

Let the supposed variation of the principal elliptic term in the Moon's longitude be $+2''\cdot2x$ in a century (i.e., x parts in 10,000); and let the supposed variation of the principal term in the Moon's latitude be $+1''\cdot8y$ in a century (i.e., y parts in 10,000), and let z'' be the (sidereal) secular acceleration of the Sun's longitude, then

$$+6''\cdot2x - 2''\cdot6y + z'' = 3''\cdot6 \pm 0''\cdot5 \quad \ldots\ldots \text{(i)},$$

the right-hand side being determined by the eclipses, and modern observations indicating that x is less than 0·2 and y less than 0·3. In this way the solar eclipses are compatible with a secular acceleration of z'' for the Sun.

If the underlying cause be tidal friction, there is nothing improbable in the suggestion that slow changes are caused in the eccentricity and inclination of the Moon's orbit.

The present tables would expose on an average about one-tenth part of the Sun's diameter, and these 200″ are reduced to less than 50″ by a single empirical term in a large number of cases. The suggestion that this is mere chance appears to me untenable. When the observations of the eighteenth century present a difficulty of one second, what is one second against two hundred? The one second may be evidence that a particular hypothesis requires modification, but that is all. I believe equation (i) exhausts every geometrical possibility.

The Perturbations of Halley's Comet in the Past.—First Paper. The period 1301 *to* 1531.

By P. H. Cowell, M.A., F.R.S., and A. C. D. Crommelin, B.A.

We commence this paper by expressing our great indebtedness to Dr. Smart, F.R.A.S.; to Mr. F. R. Cripps, of 22 Hornsey Rise Gardens, N.; and to Mr. Thomas Wright, of 39 Cringle Road, Levenshulme, Manchester; who are really entitled to be considered as joint authors, since they have carried out by far the larger portion of the mechanical quadratures, the results of which are given below. Without their co-operation the completion of the calculations would have been indefinitely delayed; and they are again offering their help in carrying them back to a still more distant date.

The calculation of the perturbations of this comet has already been carried back to 1531 by de Pontécoulant; the identity of the two apparitions before that (those of 1456 and 1378) is universally admitted; before that date Dr. Hind has given (*M.N.*, x. p. 51) a list of conjectured identifications, some fairly certain, like those of 451, 760, 1145, others admittedly vague and uncertain. It occurred to us that new light might be thrown on the question by carrying back the calculation of the perturbations as far as possible, and seeing whether a sufficiently accurate correspondence existed between the conjectured and calculated dates. It is fairly evident that, to bring the labour of computing a large number of revolutions of the comet within reasonable limits, some abbreviations must be introduced, and the following was the plan adopted.

(1) For the purpose of computing the perturbations, the comet in each revolution was assumed to move in an ellipse of constant eccentricity (experience having shown that the perturbations in eccentricity are always small), and with a major axis corresponding to the observed period.

(2) The perihelion and node were supposed to change uniformly from revolution to revolution, the rates of change being deduced from the accepted elements of the comet from 1531 to 1910; subsequently these rates were modified by the use of M. Laugier's discussion (*C.R.*, vol. xxiii.) of the apparitions of 451 and 760, for which fairly definite statements of position are found in the Chinese annals. M. Laugier has been able to represent these observations exactly by elements differing but little from those at the present time. Curiously enough, he does not print his elements, but he gives the longitudes and latitudes calculated from them, and the elements must be very approximately as follows—

(the old system of comet elements is here employed, as more convenient for our purpose) :—

T = 451 July 3·5	T = 760 June 11
$\varpi = 284°$	$\varpi = 289°$
$\Omega = 32·5$	$\Omega = 36·5$
$i = 16$ } Eq^{x}. 451	$i = 17$ } Eq^{x}. 760
$q = 0·60$	$q = 0·60$

The following are the collected values of ϖ, Ω, i, reduced to the Equinox of 1910 :—

	ϖ	Ω	i
	°	°	°
451	304·8	53·3	16
760	305·0	52·5	17
1378	306·9	54·67	17·9
1456	305·26	50·08	17·62
1531	306·47	50·77	17·00
1607	305·41	52·66	17·14
1682	305·09	54·35	17·76
1759	305·27	55·92	17·62
1835	305·55	56·19	17·76
1910	303·64	57·18	17·78
Assumed change per revolution of the comet,	−0·06	+0·15	

The secular variation of i has been neglected altogether in the period now under discussion, as it appears to be too small to deduce with certainty, considering the roughness of the elements for all the apparitions before 1682.

(3) The co-ordinates of the planets referred to the cometary axes are assumed to repeat themselves at uniform intervals, but the interval for x', y' is taken slightly different from that for z', to allow for the adopted motions of the perihelion and the node.

	Period for x', y'. d	Period for z'. d
Venus	224·7005	224·7023
Earth	365·2544	365·2614
Jupiter	4332·47	4332·86
Saturn	10758·6	10760·7
Uranus	30681·3	30700·6
Neptune	60167	60238

In consequence of the change in the period and major axis of the comet's orbit, it is more convenient to use the ordinary

astronomical unit of length and the mean solar day as the unit of time. In our previous papers on the perturbations by Jupiter and Saturn 1835–1910 (*M.N.*, lxvii. pp. 386, 511) every quantity tabulated was of zero dimensions in space and time.

(4) In the case of Jupiter and Saturn, the method of mechanical quadratures is used for values of u (the comet's eccentric anomaly) from 0° to 90°, and from 270° to 360°. For the outer half of the orbit the definite integral method described in the above paper (p. 391) is employed; since the eccentricity of the comet's orbit is treated as constant, all the values on p. 409 can be used unchanged except those relating to the position and motion of the perturbing planet; it may be noted that $\frac{x'}{a}, \frac{y'}{a}\frac{1}{na}\frac{dx'}{dt}\frac{1}{na}\frac{dy'}{dt}$ vary as $\frac{1}{a}$.

The portion of the perturbations in the outer half of the orbit that we have denoted by V_4 (p. 391) is neglected altogether; this may make an error of a few days in the calculated period, but not sufficient to cause any doubt as to the identity of an observed comet.

The intervals of u for which the calculations are made are $4\frac{1}{2}°$ for Jupiter and Saturn, 9° for Uranus, 18° for Neptune (the calculations for these two planets being made throughout the whole orbit), 1° for the Earth and Venus (the calculations for these two planets being made from the values of u 0° to 30°, 330° to 360°).

(5) We have omitted the discussion of the perturbation of e, Ω, i, having merely found the quantities that come directly into the periodic time.

The following are the assumed co-ordinates of Venus and Earth, referred to the axes of the comet's orbit; they are deduced by a simple transformation from those given by Stratford in the Supplement to the *Nautical Almanac* for 1839, and are in units of the 3rd decimal:—

Julian Day.	Venus.			Earth.		
	x'	y'	z'	x'	y'	z'
2391492	−548	−452	+122	+969	−053	−296
94	569	419	134	966	087	299
96	589	386	146	961	121	302
98	606	352	158	955	154	304
1500	624	317	169	948	187	305
02	638	279	179	941	220	306
04	650	242	189	932	253	307
06	660	204	199	922	286	308
08	668	165	208	910	318	308
10	674	125	216	898	349	308
12	678	085	224	885	380	308
14	679	045	231	871	411	307
2391516	−679	−004	+237	+856	−442	−306

Julian Day.	Venus.			Earth.		
	x'	y'	z'	x'	y'	z'
2391518	−676	+036	+242	+840	−472	−304
20	671	075	247	822	501	302
22	663	115	250	804	530	300
24	654	155	253	785	558	298
26	643	194	255	765	586	295
28	631	233	257	744	613	292
30	615	270	257	723	639	288
32	597	307	256	700	664	284
34	578	343	255	677	689	280
36	557	378	253	653	713	276
38	534	412	250	628	736	271
40	509	444	247	602	758	266
42	483	475	242	576	779	261
44	456	504	237	550	799	255
46	427	532	232	522	819	249
48	397	558	226	493	838	244
50	365	582	218	464	855	236
52	331	604	209	435	871	229
54	297	624	201	405	886	222
56	263	643	192	375	901	214
58	227	660	182	344	914	207
60	191	674	172	312	927	199
62	154	686	161	281	938	191
64	116	696	149	249	948	182
66	078	704	137	217	957	173
68	040	711	124	185	965	164
70	−002	714	111	152	972	155
72	+037	715	098	119	977	146
74	075	714	085	086	981	137
76	113	712	071	052	983	127
78	150	706	057	+019	986	117
80	187	698	042	−014	987	107
82	223	688	028	047	987	097
84	259	676	+015	080	985	087
86	294	662	000	114	982	077
88	329	646	−014	147	978	067
90	362	628	029	179	972	057
92	394	608	044	211	966	046
94	424	586	058	243	958	036
2391596	+453	+562	−072	−275	−949	−025

Julian Day.	Venus.			Earth.		
	x'	y'	z'	x'	y'	z'
2391598	+482	+537	−086	−306	−939	−015
1600	509	510	099	337	928	−004
02	534	481	112	369	916	+007
04	557	451	124	400	903	017
06	578	419	137	430	888	028
08	598	386	149	459	872	038
10	616	352	161	488	855	049
12	632	317	172	515	838	059
14	646	281	183	543	819	070
16	658	244	193	570	799	080
18	668	207	202	596	778	090
20	677	169	211	621	756	100
22	683	130	219	646	734	110
24	687	090	226	670	711	120
26	688	050	233	693	687	130
28	688	+010	239	715	662	140
30	686	−030	244	736	636	149
32	682	069	249	757	609	158
34	675	108	253	777	581	167
36	666	147	256	796	552	176
38	655	186	258	813	523	184
40	643	224	260	829	493	192
42	628	262	260	844	463	200
44	611	299	260	858	432	208
46	593	335	259	871	401	216
48	573	369	257	884	369	223
50	551	403	254	895	337	230
52	527	435	250	905	304	237
54	502	466	246	914	271	243
56	475	495	242	922	237	249
58	446	523	237	929	203	255
60	416	550	231	934	169	260
62	385	574	224	938	135	265
64	353	597	216	941	100	270
66	319	618	208	943	065	275
68	285	638	199	944	−030	279
70	250	655	189	943	+004	283
72	214	670	179	942	039	286
74	177	683	168	940	074	289
2391676	+140	−694	−157	−936	+108	+292

Julian Day.	Venus.			Earth.		
	x'	y'	z'	x'	y'	z'
2391678	+103	−703	−145	−931	+142	+295
80	065	709	133	925	176	297
82	+027	713	120	917	210	298
84	−011	715	107	909	244	299
86	049	715	094	899	278	300
88	087	713	080	888	311	300
90	125	709	066	876	344	300
92	162	702	052	863	376	300
94	199	693	038	849	408	300
96	236	682	023	835	439	299
98	271	669	−009	819	470	298
1700	305	653	+006	802	500	296
02	339	635	020	784	530	294
04	372	616	034	765	559	292
06	403	595	048	745	588	289
08	433	572	062	724	615	286
10	462	547	076	702	642	283
12	489	520	090	680	668	279
14	514	492	103	657	693	275
16	538	462	116	633	717	270
18	−561	−431	+129	608	741	265
20	...	...	...	582	764	260
22	...	...	...	556	786	255
24	...	...	...	529	806	249
26	...	...	...	502	826	243
28	...	...	...	474	845	237
30	...	...	...	445	863	230
32	...	...	...	416	880	223
34	...	...	...	386	896	216
36	...	...	...	356	911	209
38	...	...	...	326	924	201
40	...	...	...	295	936	193
42	...	...	...	264	948	185
44	...	...	...	232	959	177
46	...	...	...	200	968	169
48	...	...	...	168	976	160
50	...	...	...	136	983	151
52	...	...	...	103	989	142
54	...	...	...	071	994	133
2391756	...	...	...	−038	+997	+124

Julian Day.	Venus.			Earth.		
	x'	y'	z'	x'	y'	z'
2391758	...	...	...	−005	+1000	+114
60	...	...	...	+028	1001	104
62	...	...	...	061	1001	094
64	...	...	...	094	1000	084
66	...	...	...	127	998	074
68	...	...	...	159	994	064
70	...	...	...	191	989	054
72	...	...	...	223	983	044
74	...	...	...	255	977	034
76	...	...	...	287	969	023
78	...	...	...	318	960	013
80	...	...	...	348	949	+003
82	...	...	...	379	938	−008
84	...	...	...	409	926	018
86	...	...	...	438	913	029
88	...	...	...	467	899	039
90	...	...	...	496	883	049
92	...	...	...	524	866	059
94	...	...	...	551	848	069
96	...	...	...	577	829	079
98	...	...	...	603	810	089
1800	...	...	...	629	790	099
02	...	...	...	654	769	109
04	...	...	...	678	747	119
06	...	...	...	701	724	128
08	...	...	...	723	700	137
10	...	...	...	745	676	146
12	...	...	...	766	651	155
14	...	...	...	786	625	164
16	...	...	...	804	598	173
18	...	...	...	821	571	181
20	...	...	...	838	543	189
22	...	...	...	854	515	197
24	...	...	...	870	485	205
26	...	...	...	885	455	213
28	...	...	...	898	425	220
30	...	...	...	910	394	227
32	...	...	...	921	363	234
34	...	...	...	931	332	240
2391836	...	...	...	+940	+300	−246

Julian Day.	Venus.			Earth.		
	x'	y'	z'	x'	y'	z'
2391838	...	...	...	+948	+268	−252
40	...	...	...	955	235	258
42	...	...	...	961	202	264
44	...	...	...	966	169	269
46	...	...	...	969	136	274
48	...	...	...	972	102	279
50	...	...	...	973	069	283
52	...	...	...	974	035	287
54	...	...	...	973	+002	290
56	...	...	...	971	−032	293
58	...	...	...	968	066	296
60	...	...	...	964	099	299
2391862	...	...	...	+958	−132	−302

The co-ordinates of the comet itself are found from the table (*M.N.*, lxvii. p. 387) by multiplying $\frac{x}{a}$, $\frac{y}{a}$ by a, using the special value for each revolution, and the number of days from perihelion to each value of u by multiplying nt by $\frac{\text{period in days}}{6{\cdot}28318}$.

The co-ordinates of Jupiter are found from the table (*M.N.*, lxvii. p. 394) by multiplying $\frac{x'}{a}$, $\frac{y'}{a}$, $\frac{z'}{a}$ by 17·7. The following table gives the Julian days corresponding to certain values of g':—

g'	Julian Day.	g'	Julian Day.
°		°	
0	2390645	225	2393353
45	1186	270	3894
90	1728	315	4436
135	2269	360	4977
180	2811		

The values for intermediate degrees may be found with sufficient accuracy by reckoning 12^d per degree from the nearest table value. The co-ordinates of Saturn are found from the similar table, referred to (but not printed in detail), *M.N.*, lxvii. p. 512. As the formulæ on which the table is based are given there, the table is not printed here.

As the perturbations by Uranus and Neptune are always small, approximate co-ordinates suffice; those of Uranus are taken from de Pontécoulant (*Mémoire sur le calcul des perturbations de la comète de Halley*, 1829, pp. 37, 38, 48). They are as follows, in units of the first decimal:—

Julian Day.	x'	y'	z'	Julian Day.	x'	y'	z'
2358000	+187	+26	−55	2369400	−90	−171	+07
300	188	15	57	700	100	165	10
600	189	+04	58	2370000	109	158	14
900	189	−08	59	300	117	151	18
2359200	188	20	60	600	125	143	21
500	187	31	61	900	132	135	25
800	185	42	62	2371200	140	126	28
2360100	182	53	62	500	147	116	31
400	179	64	63	800	153	106	34
700	175	74	63	2372100	158	96	37
2361000	171	84	63	400	162	86	40
300	166	94	62	700	166	75	42
600	160	104	62	2373000	170	64	45
900	154	113	61	300	173	52	47
2362200	147	122	60	600	175	40	49
500	140	131	59	900	175	29	51
800	132	139	58	2374200	176	17	53
2363100	124	147	56	500	176	−05	54
400	116	155	54	800	175	+08	55
700	107	161	53	2375100	174	20	56
2364000	98	167	51	400	172	32	57
300	89	173	49	700	169	44	57
600	79	178	46	2376000	165	56	58
900	69	183	43	300	160	67	58
2365200	59	187	41	600	154	78	57
500	48	190	38	900	148	89	57
800	37	193	35	2377200	141	99	56
2366100	26	195	32	500	133	108	54
400	15	196	28	800	125	118	53
700	+04	196	25	2378100	118	127	52
2367000	−07	196	22	400	109	136	51
300	18	195	18	700	100	144	49
600	29	194	14	2379000	91	151	47
900	40	192	11	300	81	157	45
2368200	50	189	07	600	71	163	42
500	60	186	−04	900	60	169	39
800	70	182	00	2380200	49	173	36
2369100	−80	−177	+03	2380500	−37	+176	+33

Julian Day.	x'	y'	z'	Julian Day.	x'	y'	z'
2380800	−25	+179	+30	2385000	+124	+142	−22
2381100	14	181	27	300	133	135	25
400	−02	183	24	600	141	127	29
700	+09	184	20	900	148	119	32
2382000	21	184	17	2386200	155	110	35
300	32	183	13	500	161	100	38
600	43	181	09	800	167	90	41
900	55	178	05	2387100	172	80	43
2383200	66	175	+01	400	176	70	46
500	77	172	−03	700	180	59	49
800	87	167	07	2388000	183	48	51
2384100	97	162	11	300	185	37	53
400	106	156	14	600	187	26	55
2384700	+115	+149	−18	2388900	+189	+15	−57

The perturbations produced by Neptune are still smaller than those of Uranus, and, the eccentricity of its orbit being small, it has been considered to move in a circle. The following are its co-ordinates at intervals of 6° of longitude, or 1003 days, in units of the first decimal :—

Julian Day.	x'	y'	z'	Julian Day.	x'	y'	z'
2411217	−134	−269	00	2427265	−240	+155	+94
2220	160	254	+10	8268	222	180	92
3223	184	237	20	9271	202	204	90
4226	206	217	29	30274	179	226	86
5229	226	194	38	1277	154	245	82
6232	244	169	47	2280	128	261	76
7235	259	143	56	3283	100	275	70
8238	271	115	63	4286	071	285	63
9241	280	85	70	2435289	42	293	56
20244	286	55	76	6292	− 12	297	47
1247	288	− 24	82	7295	+ 18	298	38
2250	288	+ 07	86	8298	48	295	29
3253	284	38	90	9301	78	290	20
4256	278	68	92	2440304	107	281	+10
5259	268	98	94	1307	134	269	00
2426262	−256	+127	+95	2442310	+160	+254	−10

The co-ordinates recur with reversed signs after 30090 days.

The dates of perihelion passage in 1378, 1456 were already known with accuracy from the observations; for that of 1301 Hind's value was taken as a working hypothesis, to be tested by the result; the Julian days of the various passages are given here for convenience of reference:—

Perihelion Passage.		Julian Day.	Interval.	Corresponding log a.
			d	
1301	Oct. 22·7	2196543·70	28141·07	1·2578
1378	Nov. 8·77	2224684·77	28336·44	1·2598
1456	June 8·21	2253021·21	27471·59	1·2508
1531	Aug. 25·8	2280492·80	27811·07	1·2544
1607	Oct. 26·87	2308303·87	27351·94	1·2496
1682	Sept. 14·81	2335655·81	27936·76	1·2557
1759	Mar. 12·57	2363592·57	28006·37	1·2564
1835	Nov. 15·94	2391598·94	27199·36	1·2480
1910	May 6·3	2418798·30		

The last value is uncertain by several days; the value given was that employed in our Jupiter and Saturn work.

The following are the assumed co-ordinates of Jupiter and Saturn at the times given:—

		Jupiter.		Saturn.	
	u	x'	y'	x'	y'
1301	0°	−4·03	−3·07	−7·07	+5·25
	90	+4·88	+0·28	+5·37	+8·43
	270	−0·48	−4·95	+9·05	+3·63
1378	0	+3·46	+3·97	+3·49	−8·36
	90	−5·02	+0·58	−8·33	−2·49
	270	+1·19	+5·25	−7·44	+4·65
1456	0	−4·58	−2·09	+5·36	+8·44
	90	+4·79	−0·42	+8·51	−3·53
	270	+2·70	−3·92	+6·29	−6·55
1531	0	+0·28	+5·37	−6·60	−5·92

The method pursued has been identical with that used by de Pontécoulant in the memoir quoted above.

$x' - x$, $y' - y$ were found for each value of u; then $(x' - x)^2 + (y' - y)^2 + z'^2 = \rho^2$, where ρ is the distance between the planet and comet.

Then X, Y, the perturbing forces parallel to the axes, are proportional to

$$\frac{x' - x}{\rho^3} - \frac{x'}{r'^3}, \quad \frac{y' - y}{\rho^3} - \frac{y'}{r'^3}.$$

dn, the alteration in the mean motion, is equal to $X \sin u - Y \cos u \sqrt{1-e^2}$ multiplied by a reducing factor, which is not introduced till after the summation; each value of dn is multiplied by the number of days to elapse before the next perihelion passage.

The following quantities are then formed for each value of u:—$xY - yX$, rX, $r(xX + yY)$. In taking the sums of the columns, only half of the term at the top and bottom of each column is taken; the reason is obvious from the geometry of mechanical quadratures.

The factor for reducing $\int dn$ to seconds of arc is $\frac{6\pi}{365{\cdot}256}\, a^{\frac{1}{2}} m' du$.

The same factor multiplies the sum of $dn \times$ No. of days to next perihelion passage, and gives a quantity which we call A.

The factor to multiply

$$\int (xY - yX) \sin u - \sqrt{1-e^2} \int rX \text{ is } \frac{m' a\, du}{e};$$

the result is $\int d\varpi$.

The factor to multiply $\int r(xX + yY)$ is $2\, m' du$: this gives a quantity which we call C.

Then
$$\int d\zeta = A - \int d\varpi \sqrt{1-e^2} - C.$$

And period in days is given by the expression

$$\frac{1296000 - \int d\zeta}{n \text{ at beginning of revolution}}.$$

du is the selected interval of u expressed in seconds of arc; thus for interval—

1° $du = 3600$, log = 3·5563; for $4\frac{1}{2}$° $du = 16200$, log = 4·2095; for 9° $du = 32400$, log = 4·5105; for 18° $du = 64800$, log = 4·8116

The following are the assumed values of $\log m'$ for the different planets:—

☿ 4·3893, ⊕ + ☽ 4·4781, ♃ 6·9799, ♄ 6·4558, ♅ 5·6408, ♆ 5·7122; also

$$\sqrt{1-e^2} = {\cdot}2540, \text{ i.e. } e = {\cdot}9672, \quad \log \frac{1}{e} = 0{\cdot}0145,$$

$$\log \frac{6\pi}{365{\cdot}256} = 8{\cdot}7127.$$

The following tables exhibit the calculated quantities:—

Revolution 1456 *to* 1531.

Planet.	Limits of u.	$\int dn$.	A.	$\int d\varpi$.	C.	$\int d\zeta$.
	° °	″	″	″	″	″
Venus	0– 30	+·007	+200·9	−0·4	−1·1	+202·1
,,	330–360	+·040	+1·3	+1·9	·0	+0·8
Earth	0– 30	−·006	−164·2	−0·8	+·7	−164·7
,,	330–360	+·007	+0·6	−0·4	+·8	−0·1
Jupiter	0– 90	+1·070	+29020·	+653·9	−591·4	+29445·3
,,	90–270	+·209	...	−553·4	...	+2241·
,,	270–360	−·943	−1105·	−0·6	−1290·8	+186·0
Saturn	0– 90	−·043	−1096·	+49·0	+76·9	−1185·4
,,	90–270	+·050	...	−128·9	...	+790·2
,,	270–360	−·018	−36·6	−9·4	−42·6	+8·4
Uranus	0–360	·000	+0·1	−4·0	+14·8	−13·7
Neptune	0–360	+·002	+28·9	+6·0	+25·4	+2·0
Sums	...	+·375	...	+12·9	...	+31513·

Hence for this revolution we have the equation

$$\frac{1296000'' - 31513''}{n \text{ at } 1456} = 27471{\cdot}59,$$ the observed period in days.

Hence n at $1456 = 46''{\cdot}029$

. . . . . $1531 = 46{\cdot}029 + ''0{\cdot}375 = 46''{\cdot}404$.

The last result is in good accord with de Pontécoulant, who gives (*C.R.*, lviii. p. 708) 46″·410 for the value of n in 1531; as he has computed the perturbations by Jupiter and Saturn only for the revolutions 1531 to 1607, 1607 to 1682, his value is obviously not to be relied on beyond the first decimal.

Revolution 1378 *to* 1456.

Planet.	Limits of u.	$\int dn$.	A.	$\int d\varpi$.	C.	$\int d\zeta$.
	° °	″	″	″	″	″
Venus	0– 30	+·0147	+418·7	−1·4	−1·1	+420·2
,,	330–360	−·0019	−0·9	−1·5	−·4	−0·1
Earth	0– 30	+·0148	+417·3	−1·2	−1·4	+419·0
,,	330–360	+·0003	−0·9	−2·2	−·3	0·0
Jupiter	0– 90	−·1320	−4065·5	−611·0	−187·3	−3722·
,,	90–270	−·182	...	+16·4	...	+2991·
,,	270–360	+·6725	+636·5	−380·7	+841·1	−108·3
Saturn	0– 90	+·0593	+1553·2	−95·3	−112·1	+1689·5
,,	90–270	−·011	...	+72·8	...	+1586·
,,	270–360	−·0240	−74·5	−85·6	−53·4	+0·8
Uranus	0–360	−·0013	+17·1	+1·3	−1·6	+19·0
Neptune	0–360	−·0026	+11·2	−10·5	−153·0	+167·2
Sums	...	+·4068	...	−1101·5	...	+3452·3

Hence for this revolution we have the equation—

$$\frac{1296000'' - 3452''\cdot3}{n \text{ at } 1378} = 28336\cdot44, \text{ the observed period in days.}$$

Hence n at $1378 = 45''\cdot614$
. . . . . $1456 = 45''\cdot614 + ''0\cdot407 = 46''\cdot021$.

There are now two independent determinations of n at 1456, which are satisfactorily near each other, considering the many abbreviations that we have introduced; we are justified in adopting their mean, viz. $46''\cdot025$. The corresponding values for 1378, 1531 are $45''\cdot618$, $46''\cdot400$ respectively; the close accord of the latter value with that of de Pontécoulant is satisfactory.

Revolution 1301 *to* 1378.

Planet.	Limits of u.	$\int dn$.	A.	$\int d\varpi$.	C.	$\int d\zeta$.
Venus	0°– 30°	+ ''0023	...	– 2''0	...	+ 63''4
,,	330–360	+ ·0020	...	– 1·0	...	0·0
Earth	0– 30	+ ·0121	...	– 1·7	...	+ 357·9
,,	330–360	+ ·0074	...	+ 2·4	...	+ 0·1
Jupiter	0– 90	+1·115	+30905·	+611·2	– 721·2	+31471·
,,	90–270	+ ·1983	...	–263·6	...	+ 1303·
,,	270–360	– ·5980	– 1230·	–418·8	–1303·3	+ 180·
Saturn	0– 90	+ ·0295	+ 776·4	– 14·6	– 59·8	+ 839·9
,,	90–270	– ·040	...	–106·4	...	– 606·1
,,	270–360	+ ·0367	+ 80·9	+ 51·6	+ 76·7	– 8·8
Uranus	0–360	– ·0017	– 7·7	+ 1·0	– 6·9	– 1·1
Neptune	0–360	– ·0001	+ 7·	+ 8·6	+ 28·3	– 23·
Sums	...	+ ·763	...	–133·4	...	+33576·3

We now proceed as follows: n at $1301 = 45''\cdot618 - 0''\cdot763 = 44''\cdot855$.

Then theoretical period in days $= \frac{1296000'' - 33576''\cdot3}{44''\cdot855}$
$= 28144^{d}\cdot5$.

This only differs $3^{d}\cdot5$ from the value obtained by using Hind's identification of the comet of 1301, viz. $28141^{d}\cdot07$. The dis-

cordance is so small that we are justified in accepting Hind's result with absolute confidence. The long discussion as to the identity of the comet of 1301 is thus happily ended. Dr. Galle, in his *Cometenbahnen*, p. 155, says of the comet of 1378, "Die erste als sicher zu betrachtende Erscheinung des Halley'schen Cometen." We may now claim to have brought the "sicher" returns one revolution further back, and to have thus obtained a firm starting-point for further investigations, which have, in fact, already been commenced.

It will be remembered that Pingré, using the European observations alone, obtained elements quite unlike those of Halley; subsequently Laugier, using a combination of European and Chinese observations, obtained elements resembling those of Halley, except the position of the node, which was 90° greater. Finally, Hind rejected the European observations entirely, and showed that the Chinese ones could be well represented by the Halley elements. Our result indicates that he was justified in this course; it is rather a curious reversal of the present relations of European and Chinese astronomy. Many of Dr. Hind's older identifications rest on Chinese observations, and it is satisfactory to find that the accuracy of the latter in 1378 and 1301 is fully vindicated.

Having once satisfied ourselves of the identity of the comet of 1301, we may rewrite the last equation in the form—

$$\frac{1296000'' - 33576''\cdot 3}{n \text{ at } 1301} = 28141\cdot 07, \text{ the observed period in days.}$$

Hence n at $1301 = 44''\cdot 861$

. . . . . $1378 = 44\cdot 861 + 0''\cdot 763 = 45''\cdot 624$.

Taking the mean of this and the previously adopted value for 1378, viz. $45''\cdot 618$, we obtain $44''\cdot 858$, $45''\cdot 620$ for the adjusted values in 1301, 1378. It is advisable to make these successive adjustments in order to diminish cumulative error through quantities that we have neglected.

Observations of Comet d 1907, *from photographs taken with the* 30-*in. Reflector of the Thompson Equatorial and the Astrographic* 13-*in. Refractor at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

The following positions of Comet *d* 1907 were obtained from photographs taken with the 30-in. reflector and the astrographic 13-in. refractor, with exposures of from 20 seconds to 3 minutes.

The plates were measured in the astrographic micrometer. Six reference stars were, as a rule, measured with the comet, their positions being derived from the Catalogues of the Astronomische Gesellschaft.

The positions given are not corrected for Parallax.

Log Parallax Correction = log Parallax Factor − log Δ.

Date and G.M.T. 1907.	Apparent R.A.	Apparent Dec.	Log Parallax Factor R.A.	Dec.
	30-in. Reflector.			
d h m s	h m s	° ′ ″		
June 19 14 3 1	0 16 32·60	+ 1 14 10·8	−9·525	+0·833
July 4 14 15 1	1 14 23·48	5 49 54·7	9·516	0·817
4 14 18 47	1 14 24·23	5 49 57·5	9·512	0·817
16 14 11 38	2 22 8·84	10 28 11·1	9·540	0·805
24 14 14 19	3 22 19·51	13 42 0·7	9·562	0·802
30 14 47 11	4 15 15·97	15 44 19·6	9·564	0·792
31 14 34 8	4 24 26·04	16 0 37·8	9·573	0·799
Aug. 1 14 59 5	4 33 56·40	16 16 0·5	9·565	0·790
6 14 57 39	5 21 17·16	17 8 57·3	9·579	0·800
10 15 12 37	5 58 53·15	17 22 59·5	9·581	0·803
11 15 18 16	6 8 5·99	17 22 42·7	9·581	0·802
12 15 18 36	6 17 10·80	17 20 58·7	9·582	0·805
15 15 32 32	6 43 45·84	17 7 56·9	9·581	0·805
18 15 37 17	7 9 6·29	+16 44 44·8	−9·582	+0·811
	Astrographic 13-in. Refractor.			
20 15 46 44	7 25 20·77	+16 24 19·4	−9·581	+0·811

Royal Observatory, Greenwich:
1907 *December* 12.

Note on Photographs of Phœbe, taken at the Royal Observatory, Greenwich.

(*Communicated by the Astronomer Royal.*)

Photographs of Phœbe have been obtained with the 30-in. reflector on 16 nights, viz.—

	m		m
1907 Aug. 10	Exp. 105	1907 Oct. 2	Exp. 90
11	133	4	120
18	143	8	53
19	120	12	90
29	93	30	100
Sept. 2	53	Dec. 3	120
9	107	5	120
10	70	6	90

The photograph on October 30 was at or very near elongation, and will thus be of special value for a determination of the mass of Saturn, for which this satellite is peculiarly useful, its distance at elongation being about 28′—three times that of Iapetus.

Royal Observatory, Greenwich:
1907 *December* 13.

Occultations of Uranus by the Moon in 1908, *visible at British Observatories.* By A. M. W. Downing, D.Sc., F.R.S.

It is hoped that the publication of the following particulars will enable astronomers who are favourably situated to observe some at least of these interesting phenomena, noting any peculiarities of appearance of the planet at the time of occultation that may strike them.

Day. 1908.	Place.	Standard Mean Time of Place.		Angle from North Point.	
		Immersion.	Emersion.	Immersion.	Emersion.
		h m	h m	°	°
April 22	Adelaide	10 53	11 46	106	266
,,	Melbourne	11 25	12 20	106	266
,,	Sydney	11 20	12 18	88	284
,,	Wellington	13 10	14 18	81	288
May 19	Natal...	8 44	9 29	64	308
July 13	Perth, W.A. ...	5 41	6 35	99	275
,,	Adelaide	7 19	8 19	78	296
,,	Melbourne	7 57	8 59	79	293
,,	Sydney	8 3	8 57	58	315
,,	Wellington	10 11	11 19	62	299
Aug. 9	Cape of Good Hope...	10 4	10 55	37	321
Oct. 3	Wellington	12 20	13 7	58	287

1907 *December* 6.

The Transit of Mercury, 1907 *November* 14.

(*Communicated by R. T. A. Innes.*)

At Johannesburg, at ingress, thin clouds obscured the Sun and made observation difficult; at egress the Sun was cloudless except for a few moments just before external contact.

At Pretoria the Surveyor-General organised a party, but clouds prevented any useful observations.

The observations made here are tabulated below. The individual observers' reports are as follows:—

Mr. R. T. A. Innes, using a 9-in. refractor by Grubb, with a sun-diagonal eyepiece:—

When I looked at the Sun at $15^h\ 46^m\ 4^s$ sid. (clock) T. Mercury was already about $\frac{1}{8}$ on. At the time of internal contact rather thick clouds were passing, but the planet was never invisible. Internal contact took place, as nearly as I could estimate, at $15^h\ 47^m\ 48^s$, but it was possibly a few seconds later. Heavy clouds passed for a few seconds, making confused definition. At $15^h\ 48^m\ 1^s$ the sky cleared, but contact was passed and the planet clear of the limb by $\frac{1}{8}$ part or so. Before egress the planet was carefully scrutinised; it was a mere circular black dot, without any shading; definition was perfect, the Sun's limb being without a trace of serration. Second internal contact was at $19^h\ 8^m\ 56^s$ and was good; last contact at $19^h\ 11^m\ 36^s$ was fair, as some thin clouds were passing.

Mr. W. M. Worssell, M.B.A.A., took part in the observations with his 3-in. refractor and a direct-vision eyepiece:—

Ingress: definition was very bad, a decided line was seen at $15^h\ 47^m\ 59^s$ sid. T. Egress: suspected black drop at $19^h\ 8^m\ 48^s$, certain black drop at $19^h\ 8^m\ 51^s$; contact passed at $19^h\ 9^m\ 0^s$, external contact at $19^h\ 11^m\ 27^s$. Definition medium.

Mr. J. Innes, 3-in. refractor, direct-vision:—

Dark glass split during observations, observations uncertain. Internal contact at egress $3^h\ 46^m\ 41{\cdot}5^s$ stand. clock T., egress $3^h\ 49^m\ 1{\cdot}5^s$. One-quarter weight has been given to these observations.

Mr. H. E. Wood, M.Sc. $2\frac{1}{8}$-in. refractor (Rheinfelder & Hertel), direct-vision:—

Good definition, image small; internal contact at egress good, $19^h\ 8^m\ 56^s$ sid. T., last contact at $19^h\ 11^m\ 25^s$ more uncertain.

For the timing arrangements, electrical sounders, connected with the sidereal clock, were placed near the observers. The clock had

been rated by star observations with a transit instrument for five weeks preceding the 14th November, and had a steady rate of 0·28 seconds per day losing. Its correction at 16 hours S. T. on November 13 was +0·56 seconds. Cloudy weather prevented the observation of time stars on the 14th and 15th, but from the clock rate the corrections at the times of observations of the transit on the 14th are +0·84 seconds at ingress, +0·88 seconds at egress. The mean time clock was rated by comparisons with the sidereal clock, and its corrections are −1·3 seconds at ingress, −1·2 seconds at egress. When the corrections are applied to the above times, reduction to Greenwich mean time made, and the following reductions to the centre of the Earth applied, ingress internal contact +3·4ˢ, egress internal contact +30·9ˢ, external contact +31·2ˢ, we have :—

Observer.	Telescope.	Phase.	Geocentric Greenwich Mean Time.	
			h m s	
R. T. A. Innes ...	9-in.	II.	22 26 4·8	
W. M. Worssell ...	3-in.	II.	22 26 15·8	
R. T. A. Innes ...	9-in.	III.	1 47 7·5	
* W. M. Worssell ...	3-in.	III.	1 47 7·0	
J. Innes	3-in.	III.	1 47 11·1	¼ weight
H. E. Wood ...	2⅛-in.	III.	1 47 7·5	
R. T. A. Innes ...	9-in.	IV.	1 49 47·3	
W. M. Worssell ...	3-in.	IV.	1 49 38·4	
J. Innes	3-in.	IV.	1 49 31·5	¼ weight
H. E. Wood ...	2⅛-in.	IV.	1 49 36·4	

Results of Johannesburg observations :—

	Obs.	Nov. 1907. d	h	m	s	N.A. s	O−C. s
Internal contact at ingress	2	13	22	26	10·3	18	−7·7
,, ,, ,, egress	4	14	1	47	7·6	17	−9·4
External ,, ,, ,,	4	14	1	49	40·0	58	−18·0

The direct-vision observers using the 3-in. telescope had six sun-glasses either blistered or cracked during their observations.

* Mean of "certain black drop" and "contact passed."

Johannesburg Observatory :
1907 *November* 15.

Observations of the Transit of Mercury, 1907 November 14
By E. T. Whitelow.

Made at Birkdale, Lancashire.

Long. W. 3° 1′ 37″.
Lat. N. 53° 26′ 48″.

Neither ingress nor egress was observed, owing to clouds.

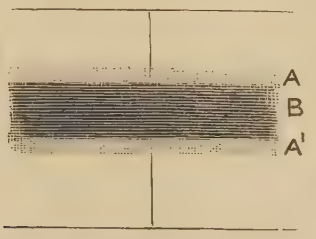
A. *Direct Observations* were made with 7⅓-in. refractor, stopped down to 4-in. A Thorp's polarising eyepiece : power 150. At 12h. 15m. G.M.T., Mercury showed a clean cut, round disc, with no indication of halo or bright spots. The duration of observation was half a minute or so of bright sunshine, and intermittent observations of a few seconds at a time when the Sun was slightly obscured by cloud, perhaps 2 minutes in all.

At 2h. 20m. there was a sudden burst of bright sunshine in a patch of clear blue sky. I put the edge of the Sun just outside the field of the eyepiece, uncrossed the prisms of the eyepiece so as to get the maximum light, and found Mercury in the field. It had much the appearance of Venus when in inferior conjunction, viz. a very thin curved line of light extending about 120° round the planet. There was no trace of this on the side away from the Sun, nor could I see *any* trace of the dark portion of Mercury contrasted against the sky.

B. *Spectroscopic Observation.* 12h. 48m. G.M.T.

Instrument 3½-in. refractor, Brashear grating, 15,500 lines per inch ; 1st order. Power 120 and 180.

The planet was observed on the Sun's disc only, with the slit parallel to the direction of diurnal rotation. Along the centre of



the spectrum was seen a shaded band, consisting of a dark central band B, bordered by slightly lighter margins A, A^1. Each was about ¼ the width of the central darker band when Mercury was bisected by the slit. To make sure these were not due to dust lines, I pulled the clock slow-motion cord to and fro, and found these margins travelled with Mercury along the slit. I also slightly increased the opening of the slit, to clear it of any possible effect from dust lines. When I moved the telescope up or down in

declination, the central dark band B became relatively narrower than the margins A, A^1. Fraunhofer lines in A, A^1 exactly corresponded with those on the Sun's surface outside these margins. They were, however, very faint, or hazy, and difficult to focus, although lines on the Sun's surface were perfectly defined. I could not say they were wide, and the difficulty of focussing seemed to be due to lack of sufficient contrast as seen on the margins A, A^1.

The total duration of the Spectroscopic observations was about $1\frac{1}{2}$ minutes of brilliant sunshine and about 2 minutes of haze.

NOTE—*November* 18, 1907.—The foregoing observations having been submitted to Father Cortie, he suggested that the failure to see a lighter ring when Mercury was *on* the Sun's disc while making the observation A might be due to the polarised field of the Thorp eyepiece being then too dark to permit it to be seen.—E. T. W.

Observation du Passage de Mercure sur le Soleil le 14 Nov. 1907.
Par Robert Jonckheere, Observatoire de Roubaix.

La situation de l'observatoire est approximativement à 50° 40′ de latitude et à 3^m 19^s de longitude Est du méridien de Paris.

L'instrument utilisé pour les observations est une lunette équatoriale de 220 mm. diaphragmée à 150 mm. La distance focale est de $2^m{\cdot}50$. Le pied est faible et les trépidations, rendent difficiles les mesures micrométriques. Le micromètre est bifilaire. Toutes les mesures ont été prises à l'aide d'un grossissement de 150, mais pour les observations visuelles des grossissements divers, variant de 70 à 600, ont été employés.

L'atmosphere pendant toute la durée du passage de la planète avait le zénith complètement dégagé de nuages mais l'horizon était brumeux. Le soleil étant à une hauteur de 30° en moyenne durant l'observation il a été par instants masqué de nuages, et c'est la raison pour laquelle le premier contact intérieur seul a pu être observé.

L'état atmosphèrique n'a pas permis de chercher à constater la présence de la planète avant son entré sur le disque du soleil.

Le 1^{er} contact extérieur n'a pu être observé et la planète n'a été aperçue que lorsque la moitié de son disque découpait le bord solaire.

Aucune déformation des cornes n'a été constatée bien que l'image se présentait à ce moment avec une grande netteté.

Aucun ligament ni pont n'a été remarqué au 1^{er} contact intérieur.

La planète était beaucoup plus noire que les taches solaires qui, en comparaison, paraissaient bleues. La corde tracée par Mercure était trop boréale pour occulter aucune de ces taches.

Aucun point obscur pouvant être considéré comme satellite n'a été observé.

Determination du Diamètre et de l'Aplatissement.

Afin de réduire le plus possible les erreurs personnelles, les mesures micromètriques ont été prises en placant l'œil horizontalement puis verticalement. Le micromètre n'a pas été fixé à un angle déterminé, il a été constamment maintenu à l'une des deux positions verticale ou horizontale.

Mesure du Diamètre Horizontal.

L'œil placé horizontalement.

	Vis 1.		Vis 2.		Mesures Différentielles.	
De 11 à 12 heures	8″·3	51″·0	8″·8	50″·4	16″·5	24″·3
	8·3	50·9	9·2	51·1	59·6	50·8
	8·2	50·5	8·0	51·2	3·9	11·0
Moyennes	8·27	9·20	8·67	9·10	7·90	

Moyenne générale 8″·63.

L'œil placé verticalement.

De 12½ à 1 heures	8″·2	50″·6	8″·7	51″·3	42″·3	31″·7
	8·0	50·4	8·8	50·8	32·4	42·2
	8·3	51·0	8·4	51·9	7·4	15·2
Moyennes	8·17	9·33	8·63	8·67	9·40	

Moyenne générale 8″·84.

Moyenne pour les deux séries de mesures 8″·73.

Mesure du Diamètre Vertical.

L'œil placé horizontalement.

	Vis 1.		Vis 2.		Mesures Différentielles.	
De 12h à 12½h	9″·8	49″·0	9″·6	51″·0	33″·5	23″·5
	9·3	50·2	9·9	51·0	31·4	21·0
	9·8	50·5	9·4	51·6	34·7	44·6
Moyennes	9·63	10·10	9·63	8·80	10·10	

Moyenne générale 9″·65.

L'œil placé verticalement.

De 1h à 1h 25m	9″·2	50″·0	9″·0	50″·6	11″·9	21″·6
	9·0	50·8	9·3	51·0	55·1	3·5
	9·5	50 2	9·1	50·7	30·7	40·0
Moyennes	9·23	9·67	9·13	9·23	9·13	

Moyenne générale 9″·28.

Moyenne pour les deux series 9″·46.

Pour l'ensemble de toutes les mesures 9″·10.

L'existence des anneaux n'a pas été constatée, mais un léger défaut de mise au point donne une image faisant croire à une apparence d'atmosphère et ressemblant entièrement aux dessins de Schroeter de 1799. La nuance de ce halo est alors identique à celle de la pénombre des taches solaires.

J'ai encore remarqué que les facules et granulations du soleil semblaient plus distinctes aux environs de la planéte : probablement l'effet du contraste produit par le disque noir.

Aucune apparence lumineuse n'a été aperçue. Le disque s'est toujours présenté uniformément noir.

Les derniers contacts allaient se produire lorsque le disque solaire disparut derrière les nuages : une éclaircie se produisit et il fut possible d'observer la planète alors qu'elle échancrait le bord solaire de la moitié de son diamètre.

P.S.—5 minutes et 6 secondes après le premier contact intérieur, un petit point noir a passé trés rapidement en face du soleil, de l'Ouest à l'Est a environ 20° de déclinaison boréale : vraisemblablement une étoile filante.

Notes.—Le phénomène dans sa marche régulière n'a donné lieu à aucune observation autorisant l'hypothèse de l'existence d'une atmosphère, même trés faible.

L'examen des mesures micrométriques montre une différence entre les diamètres vertical et horizontal de la planète.

La moyenne pour le diamètre horizontal est de 8″·73 et pour le diamètre vertical 9″·46. Cette différence de 0″·73 est surprenante. Il ne peut étre question d'une erreur personnelle ou instrumentale.

La valeur verticale 9″·46 est celle qui approche le plus du diamètre généralement adopté et cela se conçoit car les phases de Mercure ne permettent habituellement que les mesures de ce diamètre.

Il me semble également intéressant de faire remarquer que pour la première série des mesures du diamètre horizontal, la moyenne des résultats est plus faible que pour la seconde : alors que pour les mesures du diamètre vertical, c'est l'inverse qui se produit. Les mesures effectuées avec l'œil dans la position horizontale ayant été prises en premier lieu, le mouvement diurne peut expliquer cette différence étant donné le déplacement horaire de la planète dans l'intervalle des premières aux dernières mesures.

Dans le cours de mes observations visuelles, j'avais remarqué une légère déformation du disque dont l'axe était à 20° à gauche de la verticale, mais j'avais prié mon aide de ne pas noter cette particularité qui me paraissait invraisemblable. Cependant, après avoir pris la moyenne des mesures, je m'aperçus que ce n'était pas une illusion puisque les chiffres confirmaient l'apparence que j'avais constatée.

D'après ces observations l'on serait donc conduit à devoir accepter que la planète Mercure a une déformation trés sensible, et, bien qu'elle paraisse trop grande pour ne pas avoir été aperçue jusqu'ici, on peut admettre qu'elle ait échappé aux observateurs,

car ils ont bien rarement l'occasion de prendre les mesures du disque entier de cette planète.

Cette déformation pourrait servir à justifier l'hypothèse d'un mouvement de rotation dont l'axe coinciderait avec l'orbite de la planète, mais il serait peut être plus simple d'admettre qu'elle n'est pas le résultat d'un mouvement de rotation rapide, mais de l'attraction du soleil. Il faudrait alors reconnaitre avec M. Schiaperelli que cette planète n'effectue qu'une seule rotation durant sa révolution autour du soleil.

Roubaix :
le 19 *Novembre* 1907.

LUNAR NOMENCLATURE.

1. At a meeting of the International Association of Academies in Vienna in May 1907, a Committee was appointed to consider and report upon Lunar Nomenclature, with a view to securing consistency and uniformity. The Committee consisted originally of Loewy (Chairman), Franz, Newcomb, Saunder, Turner, Weiss. The names of W. H. Pickering and Puiseux have since been added, and it was hoped that an eminent German astronomer might have also joined the Committee, but he felt unable to do so. A heavy blow has fallen on the Committee in the sudden death of its Chairman, M. Loewy.

By request of the Chairman, the undersigned undertook the duties of Secretary to the Committee ; and he has accordingly received several suggestions from individual members. From these it becomes clear that it will be necessary to take a preliminary decision of the first importance :—viz. shall an essentially new system of nomenclature be propounded, or do astronomers prefer to retain simply the traditional system, with necessary corrections and possible modifications?

To propose a new system requires some courage and may only increase confusion. On the other hand, no more favourable moment than the present is likely to occur for considering such a proposal : for if the Committee succeeds in correcting and improving the present system, it will naturally be rendered more tenacious of life. Finally, the proposal for a new system need not mean the abolition of the old. In the case of minor planets we have already a double system of nomenclature—names and numbers ; and there may be advantages in having a dual system also for the Moon.

But such a new departure calls for very careful consideration ; and it would be of great help to the Committee to know whether it were likely to meet with approval. The circumstances suggest that if the Royal Astronomical Society will publish in the *Monthly Notices* the sketch-proposals themselves, so that brief comments upon them may be invited, the Committee might be greatly

assisted in deciding whether to put forward some such scheme definitely or not.

Accordingly, the present document is submitted to them for publication, containing the proposals of Dr. Franz, and the counter-proposals of M. Puiseux (to which our late Chairman had given a general approval) and of Professor W. H. Pickering. The Committee will welcome any comments made upon them by astronomers interested; and would specially appreciate replies to the following questions:—

(1) Do you approve the proposal of some mechanical or numerical system of nomenclature *in place of* the old?

(2) Do you approve the proposal of some such system *in addition to* the old?

(3) If such a system is to be adopted, which of the three proposed below seems, on the whole, preferable?

(4) Are your comments and replies intended solely for the Committee, or may they be published if thought desirable?

H. H. Turner,
Secretary to the Committee on Lunar Nomenclature.

University Observatory, Oxford:
1907 *November* 20.

Dr. Franz's Proposals.

Breslau, le 13 Août 1907.

Monsieur,—Invité à présenter des propositions pour la nomenclature systématique de la lune, je pense qu'il n'est pas trop tard pour réformer entièrement les dénominations. Car la mesure d'un grand nombre d'objets n'a été faite que depuis 1900. M. Saunder a publié les siennes pour une grande partie sans nom, et les mesures de Breslau, qui sont aussi de plus de mille, ne sont pas encore publiées et portent dans les manuscrits des dénominations provisoires. La commission ne peut se passer de publier une carte de la lune avec une nomenclature systématique, et alors dans les années et siècles à venir on aura rarement besoin de recourir aux descriptions de Mädler, qui ne se vendent plus chez les libraires, ou à celles de Neison, qui en sont un résumé.

Je m'occupe aujourd'hui seulement des cratères principaux comme Mösting, et je laisse de côté les cratères secondaires comme Mösting *A*. Les noms des astronomes, que Riccioli a introduits, n'ont aucune relation avec les cratères, et, à de très rares exceptions, jamais un astronome n'a vu le cratère qui porte son nom.

Pour avoir une dénomination systématique il faut:

(1) que les noms accusent directement la position du cratère.

(2) que les noms soient courts, afin que leurs inscriptions sur les cartes ne couvrent et ne cachent pas trop de détails.

(3) que les noms soient sonores et faciles à prononcer.

Voici, ce que je propose :

On désigne les *longitudes* (dans l'image inverse, que la lunette montre, de gauche à droite, comme on écrit) de 10 à 10 degrés par les *consonnes* : longitude plus de $+70^\circ$ $+60^\circ$ $+50^\circ$ $+40^\circ$ $+30^\circ$ $+20^\circ$

par b ch d f g k

$+10^\circ$ $+0^\circ$ -0° -10° -20° -30° -40° -50° -60° -70°* }

l m n p r s t w x z }

On désigne les *latitudes* (de haut en bas, comme on écrit) par des voyelles. Latitude plus de

-70°	-60°	-50°	-40°	par	Aa	Ae	Ai	Ao
-30°	-20°	-10°	-0°		Ea	Ee	Ei	Eo
$+0^\circ$	$+10^\circ$	$+20^\circ$	$+30^\circ$		Ia	Ie	Ii	Io
$+40^\circ$	$+50^\circ$	$+60^\circ$	$+70^\circ$		Oa	Oe	Oi	Oo

On met la consonne entre les deux voyelles.

Si dans un carré il y a plusieurs cratères à dénominer, on les ordonne par ordre de longitude et on ajoute les consonnes l, m, n, p, r, s, ... z, b . . . k, dont les premières, les plus souvent employées, sont appelées demi-voyelles par les linguistes et se prononcent facilement. Si jamais l'interpolation d'un nom nouveau devenait nécessaire, la commission pourrait facilement donner une règle d'interpolation (par exemple l'addition d'une voyelle) et je n'ai pas besoin de m'occuper maintenant de cette question spéciale.

Pour montrer cet usage, j'ai joint une liste de tous* les cratères dénominés par Neison, quoique je croie, que la commission jugera un moindre nombre nécessaire, de même qu'on préfère n'avoir pas trop de constellations dans le ciel.

J'ai aussi joint un tableau des carrés.

On n'objectera pas, que les noms nouveaux sont difficiles à se rappeler.

Il y aura partout des associations d'idées, qui aident la mémoire, et qui ne sont pas plus étrangères à l'objet que les anciens noms d'astronomes.

Par exemple Apol (Tycho) rappelle Apollon, traînées divergentes, etc.

Au sujet des cratères secondaires comme Mösting *A* ou Enom *A* je vous enverrai une troisième lettre.

* J'ai omis h, parce que les Français ne prononcent pas cette consonne.

J'ai omis q, parceque cette consonne n'a pas de son propre.

J'ai omis v, parcequ'on prononce cette consonne de même que f ou w.

J'ai placé ch au lieu de c, parceque c se prononce comme k ou comme z.

Mais je fais toutes les concessions désirées ; par exemple on peut mettre v au lieu de w (ce serait plus court) et le prononcer comme w (comme en France).

La voyelle e doit être prononcée en France comme é. Les Français peuvent écrire é, s'ils veulent.

Je pense qu'on n'aura jamais besoin d'épeler les lettres.

Je ne crains pas, que la commission trouve une difficulté en ce que les Anglais prononcent les voyelles moins sonores que les Italiens et les autres nations. On n'a pas besoin d'une désignation pour les longitudes et les latitudes qui surpassent 80°. L'aire entre 70° et 90° est plus petite que celle entre 60° et 70° dans la projection orthographique.

Agréez, Monsieur et cher Collègue, l'assurance de ma haute considération,

JULIUS H. G. FRANZ.

Boguslawsky	= afa	Pontécoulant	= achi	Fabricius	= afom
Schömberger	= aka	Biela	= adi	Lockyer	= ago
Simpelius	= ala	Hagecius	= afi	Nicolai	= ako
Malapert	= ana	Rosenberger	= afil	Barocius	= alo
Short	= anal	Vlacq	= agi	Maurolycus	= alol
Newton	= apa	Nearch	= agil	Clairant	= alom
Cabeus	= ara	Hommel	= agim	Cuvier	= amo
Klaproth	= aral	Pitiscus	= agin	Faraday	= amol
Casatus	= asa	Tannerus	= aki	Stöfler	= amom
Le Gentil	= aza	Bacon	= ali	Licetus	= amon
Boussingault	= ade	Kinau	= alil	Nasireddin	= ano
Mutus	= age	Jacobi	= alim	Saussure	= anol
Manzinus	= ake	Lilius	= ami	Pictet	= anom
Kinau	= ale	Zach	= amil	Street	= apo
Pentland	= alel	Deluc	= ani	Tycho	= apol
Curtius	= ame	Maginus	= anil	Heinsius	= apom
Cysatus	= ane	Clavius	= api	Wilhelm I.	= apon
Moretus	= anel	Röst	= asi	Longomontanus	= aro
Gruemberger	= ape	Bayer	= asil	Hainzel	= aso
Blancanus	= are	Weigel	= asim	Drebbel	= ato
Scheiner	= arel	Schiller	= asin	Schickhard	= awo
Wilson	= ase	Segner	= ati	Wargentin	= axo
Bettinus	= asel	Phocylides	= awi	Inghirami	= azo
Kircher	= asem	Pingré	= azi	Marinus	= eba
Zuchius	= asen	Oken	= abo	Adams	= echa
Bailly	= axe	Vega	= acho	Furnerius	= eda
Hausen	= aze	Steinheil	= afo	Fraunhofer	= edal
Hanno	= abi	Janssen	= afol	Stevinus	= edam

* Only a part of these are printed.

M. PUISEUX'S PROPOSALS.

Sur la proposition de M. Franz, concernant l'établissement d'une nouvelle nomenclature lunaire.

Il ne faut pas se dissimuler qu'une nomenclature entièrement nouvelle sera difficilement acceptée. Il demeurera nécessaire d'acquérir un certain degré de familiarité avec l'ancienne pour profiter des travaux descriptifs faits jusqu'à ce jour, et qui ne

pourront être remplacés ou traduits dans le nouveau système qu'après un temps assez long.

Si l'on se résout à établir un système entièrement nouveau, il doit présenter l'élasticité nécessaire pour s'étendre par degrés à des objets plus petits et moins bien délimités que ceux qui auront reçu des noms en premier lieu. Il est inévitable que les études futures amèneront à considérer de tels objets.

On ne peut pas adopter un système pour les formations qualifiées principales, un autre pour les formations qualifiées secondaires, à moins que la première catégorie ne soit extrêmement restreinte, et composée seulement d'objets familiers à tous les sélénographes.

Pour les mers, les golfes, les grands massifs de montagnes, il est à peu près inévitable que les dénominations actuelles, d'origine géographique, se maintiendront en usage concurremment avec les noms nouveaux que l'on peut souhaiter de voir adopter. Ces objets sont peu nombreux, leur nom évoque aisément le souvenir de leur position sur la Lune, ils n'impliquent nulle prétention à la ressemblance ou à l'équité, enfin ils correspondent à des habitudes déjà enracinées.

Pour les grands cirques, il faut s'attendre à voir survivre, quoi que l'on fasse, Platon, Aristote, Eratosthène, Archimède, Ptolémée, Tycho, Copernic, Képler. Tous les objets sont bien connus, très faciles à retrouver. Les noms qui leur sont donnés rappellent des illustrations incontestables, et assez anciennes pour n'éveiller aucune jalousie. Aristarque, Eudoxe, Pline, Posidonius, Euclide, Hésiode, Manilius, Proclus, se recommandent par les mêmes raisons, si ce n'est qu'ils rappellent des personnages moins célèbres ou de moindres contributions à l'Astronomie.

Les noms de savants modernes et ceux de savants anciens donnés à des objets peu apparents seront beaucoup plus facilement abandonnés, et présentent des inconvénients généralement ressentis. Leur répartition n'est pas conforme à l'équité. Une répartition nouvelle ne le serait pas non plus, ou cesserait, après peu d'années, de paraître telle. Le nombre des cirques décorés de noms d'astronomes est déjà une charge pour la mémoire, tout en restant très au dessous des exigences de la topographie. Beaucoup de cirques portent deux noms différents dans les ouvrages les plus répandus, ceux de Schmidt et de Neison. Assez souvent le nom du même astronome a été appliqué à deux cirques différents. Un changement total, en ce qui concerne les noms des cirques, a donc des chances d'être adopté, s'il est rationnel dans son principe et facile dans son application.

La proposition du Dr. Franz revient en somme à affecter une combinaison de trois lettres, facile à prononcer, à chaque rectangle de dix degrés de côté, tant en longitude qu'en latitude. Une quatrième lettre est ajoutée s'il est nécessaire.

On remarquera que la désignation du rectangle peut être faite au moyen de deux lettres seulement. Adoptant les mêmes divisions que M. Franz, on aura 16 fuseaux en longitude, 16 bandes en latitude. On leur affectera les 16 premières lettres de l'alphabet

latin, respectivement majuscules et minuscules. On aura ainsi le tableau I, dont le rétablissement n'exigera, pour ainsi dire, aucun effort de mémoire.

La désignation de ces couples de lettres sera moins dénaturée par les prononciations diverses que celle des mots de trois lettres. Elle éveillera moins souvent des associations d'idées bizarres ou triviales, et il sera plus aisé de la rétablir sans avoir le tableau sous les yeux.

Il s'agit maintenant de subdiviser les rectangles. Le Dr. Franz se sert à cet effet de bandes méridiennes, de largeur indéterminée. A chacune de ces bandes est affectée une consonne, dans un ordre qui n'est pas l'ordre alphabétique. D'où nécessité de demander un effort nouveau à la mémoire ou de recourir au tableau.

Il me semble préférable de faire suivre le couple de lettres d'un couple de chiffres. Ces deux chiffres indiqueraient le nombre de degrés comptés de l'origine du carré, respectivement en longitude et en latitude, jusqu'au centre de la formation. Ainsi le nom de Proclus pourra être remplacé par le signe Dg, 66, celui d'Hyginus par le signe Hh, 68.

Dans le voisinage des bords, on pouvrait être amené à introduire, au lieu du couple final de chiffres, deux couples de deux chiffres. Pour éviter cette complication, il vaudra mieux adopter uniformément 10° comme largeur de tous les fuseaux de longitude et de toutes les bandes de latitude. Cela conduit à introduire seulement deux lettres de plus, comme le montre le Tableau II. Le signe de Proclus devient alors Eh, 66, celui d'Hyginus Ii, 68.

Il sera superflu d'inscrire sur les cartes le couple de lettres à côté de chacune des formations comprises dans un même rectangle de 10°. Il suffira que cette indication figure une fois vers le centre du rectangle, là où se trouvera une place libre. L'inscription des chiffres sur les formations sera remplacée avec avantage par une petite chiffraison de 1 à 9, le long des parallèles et des méridiens.

Les signes proposés jusqu'ici correspondent aux points d'intersection d'un réseau divisant la surface de la Lune en rectangles de 1° de côté. La plupart des objets qui méritent des noms, mesurant un degré carré ou davantage en surface, se trouveront ainsi suffisamment désignés. On pourrait encore convenir de souligner le couple de chiffres, inférieurement s'il s'agit d'un cirque, supérieurement s'il s'agit d'une montagne isolée. On éviterait ainsi d'avoir à spécifier si l'on a entendu nommer une saillie ou une dépression.

Le cas où plusieurs points à désigner se présenteraient dans un cercle de 0°,5 de rayon, ayant pour centre un sommet du réseau sera exceptionnel. On peut cependant le prévoir. Le cercle en question pourra être divisé en 12 secteurs de 30°, désignés chacun par une des douze premières lettres de l'alphabet grec, $\alpha, \beta, \ldots . \nu$. L'ordre alphabétique suivra celui des secteurs, du Nord vers l'Est, d'après la convention adoptée pour les angles de position. On fera, dans ce cas, suivre le symbole précédemment adopté de la lettre grecque convenable. Ainsi Ii, 68 γ sera un petit cratère situé dans

un rayon de 1° environ autour d'Hyginus, l'origine de l'angle de position étant non pas le centre d'Hyginus, mais le sommet voisin du réseau.

Pour les fissures la position du centre est trop sujette à discussion. On pouvrait se contenter des numéros du catalogue de Schmidt, précédés de la seule lettre S. Ainsi S 31 sera la grande fissure d'Ariadæus. Je recommanderais, sans l'imposer, de faire suivre ce symbole de l'angle de position moyen, exprimé en degrés et mis entre parenthèses. Mais je serais d'avis de ne reporter sur les cartes que les fissures qui peuvent être tracées sans incertitudes au moyen des photographies.

En résumé je propose :

1° de ne rien changer á l'usage actuel pour les mers, les golfes, les massifs de montagnes, et pour seize grands cirques dont j'ai donné la liste ; cette liste pouvant être, si on le désire, un peu étendue.

2° de diviser l'hémisphère visible par des méridiens et des parallèles en rectangles de 1° de côté, et de représenter chaque sommet de ce rectangle par un symbole (deux lettres et deux chiffres), facile à retrouver à tout instant sans l'aide d'aucun document.

3° d'affecter aux cirques, aux montagnes isolées, aux taches, le symbole du sommet du réseau qui tombe le plus près de leur centre de gravité apparent.

4° d'indiquer par un trait, quand cela semblera utile, s'il s'agit d'une formation à centre saillant ou à centre déprimé.

5° de garder la nomenclature de Schmidt pour les fissures.

On peut facilement pousser la division de la surface plus loin par l'emploi de douze lettres grecques, ou l'étendre en longitude jusqu'aux méridiens $+100°$ et $-100°$ par l'emploi des lettres accentuées A′ et R′. Il est peu probable que l'on ait jamais à recourir à ces expédients, qui d'ailleurs ne demanderaient nulle dérogation aux conventions précédentes et imposeraient peu d'effort à la mémoire.

Les tableaux suivants (I et II) donnent la signification des symboles littéraux, suivant que l'on adopte le mode de division du Dr. Franz ou le mien. Le tableau III donne, dans la seconde hypothèse, les symboles qui répondent aux points du premier ordre. Les coordonnées adoptées sont celles de Neison. Il y aurait lieu, si ma proposition est adoptée, de revoir ce tableau en ayant égard aux déterminations plus récentes.

P. Puiseux.

1907 *November* 16.

TABLEAU I.

Longitude.		Latitude.	
+90°	A	+90°	a
80		80	
70		70	b
60	B	60	c
50	C	50	d
40	D	40	e
30	E	30	f
20	F	20	g
+10	G	+10	h
0	H	0	i
−10	I	−10	j
20	J	20	k
30	K	30	l
40	L	40	m
50	M	50	n
60	N	60	o
70	O	70	
80	P	80	p
−90		−90	

TABLEAU II.

Longitude.		Latitude.	
+90°	A	+90°	a
80	B	80	b
70	C	70	c
60	D	60	d
20	E	50	e
40	F	40	f
30	G	30	g
20	H	20	h
+10	I	+10	i
0	J	0	j
−10	K	−10	k
20	L	20	l
30	M	30	m
40	N	40	n
50	O	50	o
60	P	60	p
70	Q	70	q
80	R	80	r
−90		−90	

TABLEAU III.

Agrippa	Hi,	04	Burg	Ge,	85	Gassendi	Nk,	07
Albategnius	Ik,	41	Byrgius, A.	Pl,	44	Goclenius	Ek,	40
Alphonse, A.	Jk,	33	Campanus	Ll,	78	Grimaldi, A.	Qj,	15
Archytas	Id,	48	Capella	Mj,	58	Gueriké, C.	Kk,	22
Archimède, A.	Jg,	78	Carlini	Lm,	43	Hainzel, A.	Lu,	93
Aristarque	Ng,	73	Cassini, A.	In,	40	Halley	Ij,	68
Aristillus	If,	14	Censorinus	Fj,	20	Hansen, A.	Bh,	43
Aristote, C.	Gd,	47	Céphée, A.	Ee,	61	Harding	Qe,	13
Bessarion, E.	Mh,	75	Clavius, C.	Ko,	57	Harpalus	Nd,	42
Bessel,	Hg,	72	Cléomède, A.	Dg,	48	Hell	Jm,	82
Billy,	Ok,	04	Conon	Ig,	21	Héraclide	Me,	41
Biot,	Dl,	02	Copernic	Li,	09	Hercule	Fe,	86
Bode	Ji,	37	Crüger	Pk,	77	Herschel	Jj,	26
Bode, A.	Ji,	19	Cyrille	Gk,	34	Hésiode, B.	Kl,	77
Bode, B.	Ji,	39	Delambre	Hj,	72	Hipparque, C.	Ij,	87
Bohnenberger, A.	Fk,	97	Flamsteed	Nj,	45	Hipparque, L.	Ij,	97
Brayley	Mg,	61	Furnerius, A.	Cm,	83	Horrocks	Ij,	64
Bouillaud	Ll,	20	Gambart, A.	Ki,	91	Hortensius	Li,	86

TABLEAU III.—*continued.*

Képler	Mi,	88	Moretus	Jq,	70	Römer	Fg,	65
Lahire	Kg,	57	Mösting	Jj,	61	Rosse	Fk,	48
Lalande	Jj,	94	Mösting, A.	Jj,	53	Sacrobosco	Hl,	64
Landsberg	Lj,	60	Murchison, A.	Ij,	14	Scheiner, A.	Lp,	70
Landsberg, A.	Mi,	10	Mutus	Gp,	93	Schubert, A.	Bi,	73
Langrenus	Cj,	18	Olbers	Qi,	88	Seleucus	Pg,	61
La Peyrouse, A.	Bj,	49	Parry, A.	Kj,	69	Struve, B.	Ce,	53
Laplace, A.	Le,	73	Petavius	Dl,	95	Taruntius	Ei,	66
Le Monnier, A.	Gg,	96	Phocylides, E.	Oo,	65	Thalès	Ec,	92
Lichtenberg	Pf,	71	Picard	Dh,	44	Thebit, A.	Tl,	61
Lindenau	Gm,	42	Piccolomini	Fl,	29	Théophile	Gk,	61
Linné	Hg,	28	Picon	Je,	95	Timocharis,	Kg,	37
Mädler	Gk,	91	Pitiscus	Fo,	00	Tycho	Kn,	23
Maginus	Jo,	70	Pline	Gh,	35	Ukert	Ii,	18
Manilius	Ih,	94	Posidonius, A.	Gf,	92	Vega, A.	Cn,	95
Marius	Ok,	02	Proclus	Eh,	76	Viète, A.	Om,	73
Maskelyne	Mi,	03	Ptolémée, A.	Jj,	19	Vitello	Mm,	70
Maurolycus,	Hn,	43	Pythagore	Pc,	23	Vitruve	Fh,	18
Mayer	Lh,	96	Pytheas	Lg,	10	Werner	Il,	38
Menelaus	Hh,	66	Ramsden	Mm,	22	Wichmann	Mj,	28
Messier	Ej,	72	Reinhold	Li,	33	Wollaston	Nf,	70
Milichius	Mh,	00						

PROFESSOR W. H. PICKERING'S PROPOSALS.

It does not seem to the writer practicable to abolish the system of names at present applied to the chief lunar craters. A supplementary system of nomenclature for designating the minor points, however, seems to him very desirable. He would therefore make the following suggestions to the Committee on Nomenclature.

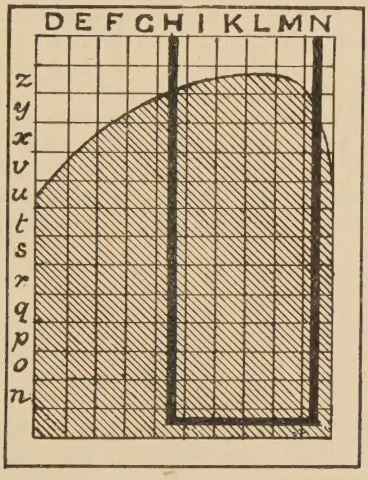
A series of photographs should be taken, or, if practicable, negatives already taken should be used. These should be enlarged through a reticule to a uniform scale of 400 mm. to the Moon's diameter. The positives should be on plates measuring 20 × 25 cm. The enlargements should be made through a reticule of squares, each square measuring 20 mm. on a side. These positives would form a set of standards which should be safely stored. From them negatives should be printed which could be furnished to engravers, photographers, or the public. The reticule should be oriented parallel to the Moon's axis.

Since it is desirable to get all the detail possible, and since the maximum detail is shown only near the terminator, it is proposed

to use photographs taken at eight different ages of the Moon, one set representing the northern hemisphere and the other the southern. This will give us sixteen different photographs. Each of these should be selected in as favourable a libration as possible.

Besides the lunar detail which shows near the terminator, there is an entirely different kind of detail, consisting of bright and dark surfaces, and of bright points which show only when remote from the terminator. Six plates should be taken to show this detail, three for each hemisphere. These would be taken when the terminator is distant, but not necessarily at full moon. This would give us a class of detail which has not been so carefully studied as that at the terminator, and for this reason is of all the more importance.

We thus have in all twenty-two standard plates. While it would be desirable to have an atlas engraved from them which



could be issued to the public, this would not be necessary, since a photographic edition of the whole twenty-two would not be very expensive. It should be possible for those interested to buy photographs of a single region.

The positive standards should be lettered in capitals running from the western to the eastern limb, and in small letters running from north to south. Twenty-four letters should be used, j and w being omitted. Around the selected region of each standard a heavy black line should be drawn, as shown in the figure, and the regions outside of this should not be used. The selected regions should overlap slightly on the different plates, which the use of twenty-four letters will permit. Otherwise only twenty letters would be needed. Beginning at the western limb, two capitals should be assigned to the first plate, three to the second, and three to the third. Four capitals will be assigned to the fourth plate, as

shown in the sketch, and the same arrangement will apply to the other quadrants.

Any area of the Moon's surface can now be indicated by two letters. Thus Kq will be found on only a single sheet of the atlas, and its exact area will be there clearly shown. The six sheets taken away from the terminator, under high illumination, could be indicated by the use of Greek letters.

The sides of the squares may be divided to tenths by the eye, and the position of any point within the square indicated thus, "Kq 63," indicating that the point measures ·6 way from the side between J and K, and ·3 way from the bottom of the square. Should still greater accuracy of designation be required, a millimetre scale could be used, and the point designated thus, "Kq 65, 37." The squares measure approximately 100″ on a side, so that this system should give us all the accuracy needed.

A double catalogue would go with the atlas, one part giving the old nomenclature of every point hitherto named in terms of the new, and the other giving the new in terms of the old. It is not intended that this nomenclature should supersede the older one; the idea is simply to furnish one which shall be purely supplementary, and absolutely free from ambiguity. It could be made readily accessible to all, so that for purely scientific work anyone could use it who chose to do so; and since the older nomenclature would also be given in the catalogue, no one need feel compelled to use the supplementary system who did not like it.

WILLIAM H. PICKERING.

1907 *November* 8.

MR. SAUNDER'S PROPOSALS.

I have already expressed my views as to the best method of dealing with the problem presented by the present condition of Lunar Nomenclature in *R.A.S. Monthly Notices*, lxvi. p. 41, and in a note printed in view of the submission of the question to the Vienna Congress. It might, however, be convenient if I summarise them here.

I am not in favour of any radical change. So much work of permanent value has been published in terms of the old nomenclature that it can never be entirely superseded, and to create another, to be used concurrently with it, would, in my opinion, only increase the confusion.

The present nomenclature, however, does require careful revision in order to ensure that—

(1) No two formations should have the same name.

(2) No formation should have more than one name.

In assigning names for future use I would suggest the following general rules—

(1) That Mädler's principal names be retained.

(2) That where Mädler has not given principal names, as little alteration as possible be made in names now generally accepted. These rules should not prevent the Committee from rejecting a name altogether where none seems required, or from introducing a new one where one would be advantageous.

(3) For minor formations only two alphabets should be used; the capital Roman to denote craters, depressions, and dark areas; the small Greek to denote peaks and bright spots.

(4) Such rills as are in Schmidt's catalogue might be denoted by their catalogue numbers, others by a short reference to their position.

I have always thought that the best method of publishing the decisions of the Committee would be to embody them in an accurate map, which must be specially prepared. But the practical difficulties appeared so great that I suggested the use of photographs, very much as Professor W. H. Pickering now does, though I had not thought out the arrangement in so much detail.

Dr. Franz has, however, in a recent letter, suggested the construction of an atlas giving accurate outlines and positions of all the more prominent features, but not presenting the minute detail. The different parts of the surface would be represented in a series of maps, and he has offered to be responsible for the limb regions if I would undertake the central parts. Dr. Franz has made a special study of the limb, and has accumulated a large number of unpublished measures. His maps would be constructed under favourable librations of 10°. I have a large number of measures of the central regions besides those already published; and as Mr. W. H. Wesley has kindly placed his great skill as a draughtsman at the disposal of the Committee for the delineation of these parts, it seems to me that the construction of such a map is now quite feasible. The maps of the central regions would be under mean libration.

I believe that with the data now available a map might be constructed in which the positions of all objects shown would be accurate to within less than 1″ of arc (geocentric), corresponding to less than one mile on the surface of the Moon for the central part, whilst the measured points are so thickly distributed that an object not represented in the map, but whose position could be located by cross bearings from points that are represented, could have its co-ordinates read off to very nearly this degree of accuracy.

With such a map it would be possible to refer to unnamed formations by means of their selenographical co-ordinates, and I believe this to be the best solution of the problem that has yet been suggested.

S. A. Saunder.